
A DEO
ET
REGE

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Preſent Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Conſiderable Parts of the WORLD.

VOL. XLIV. PART II. For the Year 1747.

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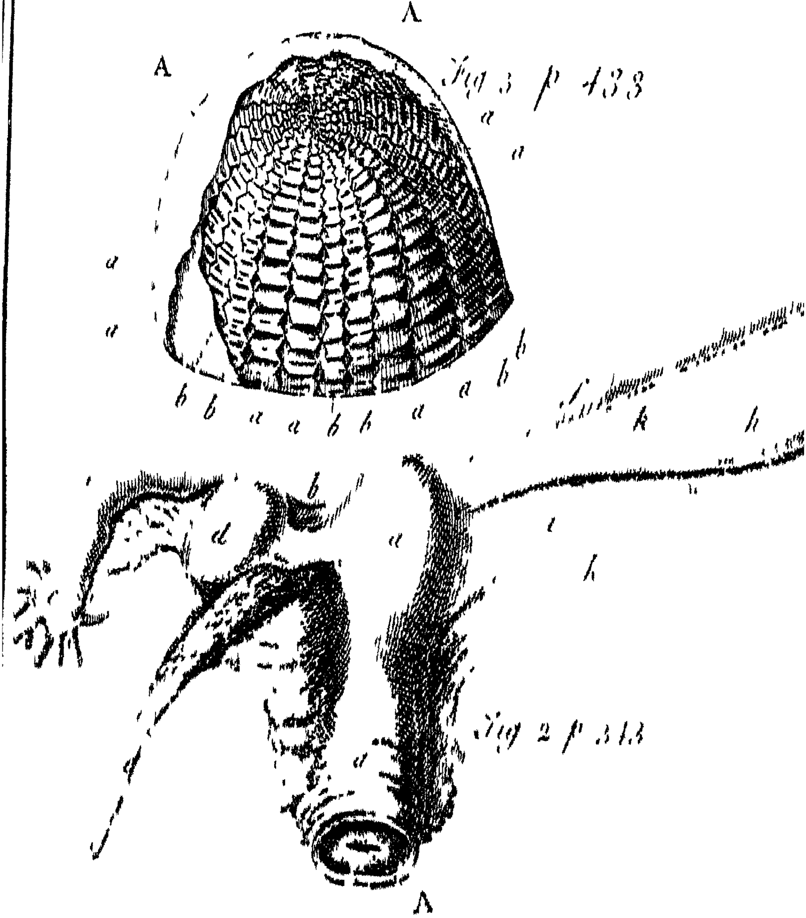

Fig 3 p 433

Fig 2 p 313

1 2 3 4 Inches

Numb. 482

PHILOSOPHICAL TRANSACTIONS.

For the Months of *January* and *February*, 1747.

The CONTENTS.

ERRATA.

[illegible] 299. l. 9. *for* $\frac{2}{10}$ *read* $\frac{4}{10}$. *for* [illegible] 2.

I. *A Letter from Mr.* Wm. Fidge, *Surgeon, at* Portſmouth, *to* Cromwell Mortimer, *M. D. Secret. R. S. concerning a* Stone *taken out of the Bladder of a* Dog; *which being cut aſunder had a Piece of* Dog-Graſs *in its Center.*

S I R,

Read at a Meeting of the Royal Society, Jan. 8. 1746-7.

ACcording to your Directions I have ſent an Account of the Stone I left with you; It is now fourteen Years ſince I took it out of the Bladder of a very large Maſtiff, about five Years old, belonging to the Porter of his Majeſty's Dock-Yard at *Portſmouth*. The Dog died in about three Days after receiving a Kick from ſome one endeavouring to part him from another Maſtiff he was fighting with.

Being then an Apprentice to the Surgeon of the Yard, and hearing the Dog was dead, I ſent the Labourer, who attended the Surgery, to get him for me, in order to diſſect.

When I had open'd the *Abdomen*, I found it fill'd with bloody Urine; and having before heard that his Death was ſuppos'd to be occaſion'd by the Kick, I immediately thought the Bladder muſt be the Part hurt; which, when I had cleanſed the *Abdomen*, I examined, and found this large Stone, with the Bladder contracted cloſe to it on every Side, and rent at the Bottom about three Quarters of an Inch; ſo that what Urine came to the Bladder was diſcharg'd

into the *Abdomen*; which was plainly the Cause of his Death.

When I first took it out it weigh'd 10 Ounces 2 Drachms and an half. It is not more than two Months since I cut it asunder; when, finding it form'd upon (as I imagine it it is) a Piece of Dog Grass, I thought it would not be an unacceptable Present to the Curious; therefore, having some Affairs which call'd me to Town, I brought it with me for that Purpose.

What is to be farther remarked is, that I did not find any the least Particle of Gravel or Sand either in the Kidneys or Ureters; and that all the Bones (except the Ribs and *Cranium*) are more or less carious, as the Bones you have with the Stone.

If, when you shew it to the *Royal Society*, it should be thought deserving a Place among their Curiosities, it will give me the greatest Pleasure, in having this Opportunity of presenting something worth the Notice of so Learned and Ingenious a Body of Gentlemen. I am,

S I R,

With all due Respect,

Your most obedient,

London, Dec. 29. 1746.

humble Servant,

William Fidge.

II. *An uncommon* Dropſy *from the Want of a Kidney; and a Deſcription of a large* Saccus *that contain'd the Water, by* Samuel Glaſs, *Surgeon, at* Oxford; *ſent to Dr.* Mead.

November 11, 1746.

Read Jan. 8. 1746-7.

MARY Nix, who lived at *Hampton-Poyle*, a ſmall Village in *Oxfordſhire*, had been remarkable all her Life for the preternatural Size of her Belly. After her Death, I had the Curioſity, together with ſome learned Gentlemen of the Univerſity, to inſpect her Body. Her Mother was then preſent, and inform'd us, that this her Daughter was born dropſical; that ſhe herſelf had been ill of the ſame Diſeaſe for ſome time before, and during, her Pregnancy; but, on the Birth of this Child, ſhe was freed from that Diſorder.

The Child, tho' born dropſical, prov'd otherwiſe healthy; and, notwithſtanding the Diſeaſe continually increaſed as ſhe grew up, liv'd to be near twenty-three Years of Age.

She was a tall well-proportion'd Woman, except with Regard to the enormous Size of her Belly; and, for one of ſo unwieldy a Bulk, healthy, briſk, and active. Her Appetite was always good, and ſhe was never more than ordinarily thirſty; had no remarkable Difficulty of Breathing, not even when ſhe lay ſupine, nor did her Thighs or Legs ever ſwell. Her *Menſes*, which appear'd at the uſual Time of Life, continued regular, till within eight Months of her Death. The only Complaint was now-and then a

Pain in making Water; and the Quantity she made was commonly about four or five Ounces.

Upon the Suppression of her *Catamenia*, there succeeded a *Dyspnœa*, Loss of Appetite, Emaciation of the superior Parts, and a Tumefaction of one of her Legs with Ulcerations. These Symptoms gradually increased till her Death.

Upon taking the Dimensions of her Body before Dissection, we found the Circumference of her *Abdomen* to be just six Feet four Inches, and from the *Xyphoid* Cartilage to the *Os Pubis* it measur'd four Feet and half an Inch. The cutaneous Vessels, distributed upon the *Abdomen*, were remarkably large, and distended with Blood, and the spurious Ribs were pressed greatly outwards and upwards.

After this general View of the external Parts, we began the Dissection, by dividing the Cartilages of the six superior Ribs, and raising the *Sternum*. The *Thorax* being laid open, we observed that the Diaphragm was forcibly protruded into that Cavity. The Base of the Heart lay under the right Clavicle, and its *Apex* upon the most convex Part of the Diaphragm; which Convexity advanced as high up as the third superior Rib. The Lungs were surprisingly small, scarce exceeding in Magnitude those of a new-born Child. The right Lobe slightly adher'd to the *Pleura*, the left was free, and both were in a sound State. Within the *Pericardium* was found, as usual, a small Quantity of Liquor, but none in the Cavity of the *Thorax*.

We next perforated the *Abdomen* in the most convenient depending Part, and evacuated from thence a surprising Quantity of Water, which was lightly tinged

tinged of a Coffee-Colour, limpid as Urine, and not in the leaſt fetid. This Water was carefully meaſur'd, and found to be not above a Pint leſs than thirty Gallons Wine Meaſure; which muſt weigh, according to the common Calculation, near 240 *l.*

We afterwards made an Inciſion into the *Abdomen* along the *Linea alba.* The Integuments upon the epigaſtric Region were very thin; the abdominal Muſcles much extenuated; and above the *Umbilicus* the *Tunica celluloſa* contain'd no Fat; but from the Navel to the *Os Pubis*, the *Panniculus adipoſus* was half an Inch thick. Upon dilating the Inciſion, the large membranous Bag that contain'd the Water preſented itſelf to View, adhering tranſverſly about ten Inches to the anterior Part of the *Peritonæum.*

This Adheſion being ſeparated, we had a full View of this wonderful Reſervoir, which was of an enormous Size, and had almoſt occupied the whole Cavity of the *Abdomen.* In Figure, Colour, Thickneſs, Number, Magnitude, and Diſtribution of Blood veſſels, it very much reſembled the *Uterus* of a Cow at the End of Geſtation. The whole Inſide was ſcabrous, and look'd as if parboil'd; and here and there was obſerv'd a ſmall Quantity of a Coffee-colour'd Sediment. On the left inferior Part was diſcover'd the Orifice of a Duct, which open'd obliquely into the Cavity of the *Saccus*, and would eaſily admit of a large Gooſe-Quill. From this Opening the Tube advanc'd about twelve Inches between the Membranes of the Bag obliquely upwards, and towards the right, from whence it was inflected downwards, and paſs'd between the Duplicature of the

the *Ligamentum latum Uteri*, to be inserted into the Bladder of Urine. The *Saccus* was connected to the *Ligamentum suspensorium Hepatis*, to a considerable Part of the *Mesocolon*, to the *Peritonæum* on the right Side in two or three different Places, to the same Membrane the whole Length of the Spine, and to the *Ligamentum latum Uteri* on the right Side of the Body.

The Liver was sound, but less than in a natural State; and its convex Part adher'd closely to the Diaphragm. The Stomach, Spleen, *Omentum*, small Intestines, and the upper Part of the *Colon*, were thrust very high up into the left *Hypochondrium*. The Convolutions of the lower Part of the same Intestine were intirely obliterated; and that, together with the *Rectum*, formed one continued strait Tube, from the left *Hypochondrium* down to the *Anus*. The left Kidney, with its emulgent Vessels and *Ureter*, were in their natural State and Situation. The *Uterus*, *Tuba Fallopiana*, and *Ovarium*, on the same Side, had nothing preternatural; but, on the right Side, the *Fallopian* Tube and Ovary were dispos'd in a very extraordinary Manner. The Tube, by means of the Adhesion of the *Ligamentum latum Uteri* to the *Saccus*, was extended to three times its ordinary Length. The Ovary was likewise, by the same Cause, render'd very preternatural, being no less than five Inches three Quarters long, one Inch broad, two Tenths of an Inch thick, and two Inches and half distant from the *Uterus*. The Bladder of Urine was very small, but appear'd to be sound.

We then made an accurate Search for the right Kidney; but, to our great Surprize, found no such

ſuch *Viſcus*, nor any thing analogous to it, unleſs the *Saccus* that contain'd the Water already deſcrib'd, may be eſteemed ſuch: And what ſeem'd to favour this Opinion, was the Diſpoſition of the emulgent Veſſels on the right Side, which were propagated from the *Aorta* and *Vena cava* to this *Saccus*, in the ſame manner as to the Kidney on the oppoſite Side; and, after having ran twelve or fourteen Inches between the Membranes of the Bag without any Ramifications, were diſtributed all over it in the Manner before-mention'd.

From the foregoing Account the following Queries are naturally ſuggeſted, which I leave to the Determination of the Learned:

Query 1. Was not the *Saccus* originally a mis-ſhapen Kidney, and the Duct a *Ureter?*

Query 2. Was not the Water contained in the *Saccus* prevented from growing putrid, by being continually drain'd off thro' the Duct into the Bladder of Urine, and by being afreſh ſupplied by the emulgent Artery; and more being ſecreted than was evacuated, the Quantity thereby continually increaſed?

Query 3. Was not this the Reaſon why the Patient had never any anaſarcous Swellings of her Thighs or Legs, nor any Thirſt, or other Signs of a confirm'd Dropſy?

Query 4. Were not the Lungs prevented from growing by the great Diminution of the Cavity of the *Thorax*, and the Preſſure they ſuſtain'd from the diſtended *Abdomen?* And might not their never having occupied a larger Space than they did at Birth, be the Reaſon ſhe never labour'd under any Difficulty of Breathing?

Query

Query 5. Was not the Bladder of Urine likewiſe by the ſuperincumbent Weight, prevented from dilating itſelf; and that the Reaſon why the Water was often made, and always in ſo ſmall a Quantity?

In order to convey a more clear Idea of the ſeveral Parts already deſcrib'd, to thoſe who may not have an Opportunity of inſpecting them (they being properly preſerved for that Purpoſe), and being ſenſible of the great Difficulty of clearly repreſenting by Words ſuch Things as are out of the ordinary Courſe of Nature, I muſt beg Leave to reſer the Reader to the Figures hereto annex'd.

See TAB. I.

FIG. 1.

aaaa, The great *Saccus* that contain'd the Water.
bb, The greater Diameter of the *Saccus*.
cc, The leſſer Diameter.
d, A prick'd Line ſhews the Entrance of the emulgent Veſſels.
ef, Shews the Courſe of the Duct between the Membranes of the *Saccus*.
f, The Orifice opening obliquely into the Cavity.
eg, The inflected Part of the Duct, after its Egreſs from the *Saccus*, which paſſed between the *Ligamentum latum Uteri* to the Bladder of Urine.
g, The Duct, divided near the Bladder, where its Cavity was ſo ſmall as only to admit of a common-ſiz'd Probe.

FIG.

FIG. 2.

Represents the posterior View of the *Uterus*, *Fallopian* Tubes, and Ovary.

aa, The *Uterus*.
A, The *Os Tineæ*.
bc, The *Fallopian* Tube in a natural State
c, The *Morsus Diaboli*.
d, The left Ovary in a natural State.
ee, The *Ligamentum rotundum*.
fg, The right *Fallopian* Tube.
g, The *Morsus Diaboli*.
hh, The right Ovary.
ii, The *Ligamentum rotundum*.
kkkk, The *Ligamentum latum Uteri*.
ll, Its Adhesion to the *Saccus*.

III. *An Explanation of an* ancient Inſcription *diſcovered at* Rutcheſter, *the laſt Station in* England, *upon the* Roman Wall, 1744; *by* John Taylor, *LL D. Chancellor of* Lincoln, *and Regiſter of the Univerſity of* Cambridge.

Read Jan. 15. 1746-7

IMP. CAES. M. AVRELIO.
SEVERO. ANTONINO.
AV. PIO.FELICI.✕.PARTHIC.
i. e. MAX.BRIT.MAX.GERM.
Auguſto. MAX.PONTIFICI.MAXIM.
TRIB.POTEST.XVIII.IMP.II.
COS.IIII.PROCOS.P.P.COH.I.
FIDA.VARDVL.CREO⊗ANO
NNANA.FECIT.SVB.CVRA.TCO
LEG.XX.GR.

According to the Copy given me by Dr. *Hunter* of *Durham,* who copied the Inſcription this laſt Summer.

This Inſcription addreſſed to *Caracalla* has nothing in it very ſingular, except the Title of the Cohort that dedicated it, namely,

FIDA.VARDVL.CREO⊗ANONNANA.

concerning which I ſhall offer theſe few Conjectures. The

The *Varduli* were a People of *Hispania citerior*, mentioned by *Pomponius Mela* and others; and are recorded now, in no less than three Inscriptions, to have served in *Britain* as Auxiliaries. The two other are printed in *Horsley*'s *Britannia Romana*, *Northumb.* N°. XCIV$^{\delta}$. *Durh.* XXVI. We find Troops of several Nations to have been here upon the same Occasion, as *Cohors prima*, *secunda*, *&c.* *Batavorum*, *Dacorum*, *Nerviorum*, *Tungrorum*, *Delmatarum*, *Thracum*, *&c.*

This Cohort of the *Varduli* is intituled FIDA, a very common Appellation, and moreover CREO⊗A-NONNANA; the last Letters of which I separate, and read without inserting a single Letter ANTONINI-ANA, thus, ANTONNANA. Nothing is so frequent in Inscriptions, as this compendiary Way of writing ANTONINVS, and its Derivatives. Thus in *Horsley*'s Inscriptions,

M.AVR.ANTONNO.PIO.

North. CXIII.

M.AVRELI.ANTONINI.PII.FELIC.

North. XCIV$^{\delta}$.

COH.IIII.BR.
ANTONNIA.

North. LXXVI.

Upon this Hint therefore I am persuaded, that when the Stone is next inspected, these little *Apices* will appear, which are easily overlook'd, when this brief Manner of Writing is not expected or attended to.

In regard to the Appellation *Antoniniana*, it is observable of the ancient Militia, that several of their Cohorts and Legions, as well *Roman* as Provincial, complimented themselves with the Imperial Surname, of which I shall produce some Instances.

DIS. MANIBVS.
C. ANTONI. C. F.
OVF. AEGRILLI.
MIL. LEG. VII. CLAVDIAE.
PIAE. FELIC. ET. FIDEL.
VIX. ANN. XXXIIX.
M. VII. D. VIII.
M. AEGRILIVS. FRATRI
DVLCISSIMO. F. C.

Gruter. DXXI. 4.

I. O. M.
COH. I. AEL.
DA. GORD
. ANA.

Horsl. Cumberl. VIII.

i. e.

Iovi O*ptimo* M*aximo* COH*ors* I*ma*. AEL*ia* DA*corum* GORDIANA.

NVMINI FOR
TIS. FORTVNE.
M. AVRELIVS.
MARINVS. MIL.
COH. VII. PR. P. V.
SEVERIANAE.

Gruter. MXIII. 10.

i. e.

COH*ortis* VII*ma* PR*ætoriæ* P*iæ* V*ictricis* SEVERIANAE.

But

But the Imperatorial Addition ANTONINIANA is perhaps the most frequent of any, as the Name of *Antoninus* was assumed by a long Series of Emperors.

DEAE. EPONAE. M. OPILIVS. RESTIO.
MILES. LEG. XXII. ANTONINIANAE. P. P. F.

Gruter. LXXXVII. 4.

H. D. D.
DEABVS. MATR.
IVLIVS. REGVLVS. MI
LES. LEGIONIS. VI.
ANTONINIANE.

Gruter. XCII. 2.

D. M.
MARINA. SECVN
DINA. VIX. ANN. LIII.
L. AVRELIA. SATVR.
VIX. ANN. XXI.
L. AVRELIVS. INGENVVS.
MILES. LEG. XIII. GEM.
ANTONINIANAE.
MATRI. ET. SO
RORI. POSVIT.

Gruter. DXXVIII. 8.

D.

D......
ET. MEMORIAE.
AETERNAE. VE
GETINIAE. RO
MANAE. MEM
MIVS. RVSTICVS.
MILES. LEG. V.
VICTRICIS. AN
TONINIAN. CON
IVGI. INNOCENTISSI
MAE. LOCO. PERE
GRINO. DEFVNCT.
P. C. ET. SVB. ASCIA. D.

Reinef. VIII. 68.

But more particularly the two following, as they both bear Date under *Caracalla*; and one of them the very Year of his fourth Confulate, when he was collegued with *Balbinus*, and of confequence nearly coæval with ours. This is to be feen in *Gruter*, XLIV. 2. and runs thus:

P. AEL. VICTORINVS. ROMANI. VIVENI. F.
AVRELIANA. POPA. MIL COH. V. PR. ANTONINIA
NE. P.V.7.VERI. HERCVLI. DONV. POSVIT. LIBI
ENS. VOTO. VT. GRATIAS. AGO. DEDICAVIT.
III. NON. NOVEMBRES.
IMP. ANTONINO. AVG. N. IIII. ET. D.
CAEILIO. BALBINO. II. COS.

The other is fomewhat earlier, and bears Date between his fecond and third Confulate.

IMP.

IMP. CAES. M. AVRELIO.
ANTONINO. AVG. SEVERI. F.
TRIB. POT. VIIII. COS. II.
COH. I. VIG. ANTONINIANA.
CN. RUSTICVS. RVFINVS. PRAEF. VIG.
TIB. CLAVDIVS. MARCELLINVS. TRIB.
etc.

Reineſ. III. *ult.*

There is yet a third, which, beſides the Title *Antoniniana*, and the Correſpondency of Date (for it was erected in the Conſulate we have been ſpeaking of) is likewiſe remarkable for the ſame *Scriptura compendiaria.* I ſhall give it whole from *Gruter*, CVIII. I.

IN. H. D. D.
GENIO. SANC
TO. M. AVREL.
CL. POMPEIAN
VS. MIL LEG. VIII.
ANTONINIANAE
AVG. B. F. COS. K.
IANVAR. IMP. D. N.
ANTONINO. IIII. ET.
BALBINO. II. COS.

What remains to be accounted for is CREO⊕. Which Letters if any one ſhould compare with *Horſley*'s *Durham* Inſcription, N°. XXVI. where mention is made of the ſame *Varduli*, he will find a very ſtrong Reſemblance;

Resemblance; and be apt to conclude, that what explains the one, will bid very fair to explain the other. That Inscription runs thus,

. O. M.
.... ATI. COH
VARDVLOR.
CREQ∞
V.S.L.L.M.

CR then I take to be distinct Marks, and expressive of *Civium Romanorum*. And of this I find little Room to doubt, when I observe the same Marks applied to several Corps, who were as strictly Provincial as our *Varduli*; such as *Afri*, *Asturienses*, *&c.*

L.PRAESENTIO.L.FIL.
LEM.PAETO.
L.ATTIO.SEVERO.
PRAEF.COH.I.AFR.
C.R.EQ.IVDICI.SELECTO.EX.
V.DEC.PR.AVXIMI.PAT.COL.
AEDILI.II.VIR.ANCONAE.

Gruter, CDLIX. 9.

SEXTL.CLASENO ......
.... PRAEF.COH.
III.ASTVR.EQ.C.R. *etc.*

Gruter. CCCLXXXVIII. 3.

DIANAE

DIANAE
SACRAVIT
T.FL.
ITALICVS
PRAEF.
ALAE.I.VLP.
CONTAR.
∞.C.R.

Gruter. XL. 3.

DEIS DEABVS.
ALA.I VLP.CONT.
∞. C.R.CVI.PRE.
Q. ER. INGENVVS.

Gruter. II. 6.

But ſtill more expreſly,

.... CEREALI.
...LEG.III. GALLICAE
...III.COH.GALLICAE.
*civ*IVM.ROMANORVM.

Gruter. CDXCIX. 3.

And moreover what kind of Citizens, we are told more than once; *viz. Juris Italici.*

T.FL.BARSI.V
ETER.ALAE.I.FL.
AVG.BRIT.
∞ .C.R.IVRIS.ITALICI.
MEMOR. FR
ATRI.SVO.POSVIT.

Gruter. DXLI. 8.

T.F.VERECVND.
MAG.EQVES.ALAE.
I.FLA.AVG.BRIT. ∞.
C.R.IVR.ITALICI.

Gruter. DXLII. 7.

The Freedom of the City had been for some time before this a regular Reward for the Fidelity of the Provinces, or any other military or civil Merit. The famous Oration of the Emperor *Claudius*, or the Act for incorporating the People of *Vienne* in *Gaul* (a large Fragment of which is preserved in *Gruter*, p. DII.) is a remarkable Instance of what I have advanced. QVID. ERGO. NON. ITALICVS. SENATOR. PROVINCIALI. POTIOR? EST. IAM. VOBIS. CVM. HANC. PARTEM. CENSVRAE. MEAE. APPROBARE. COEPERO. QVID. DE. EA. RE. SENTIAM. REBVS. OSTENDAM. SED. NE PROVINCIALES. QVIDEM SI. MODO. ORNARE. CVRIAM. POTERINT. REIICIENDOS. PVTO. For so the last Words must be emended, as they have been restored by *Reinesius* and *Grævius*.

Afterwards, probably a little before the Date of our Inscription, which is near the End of the Reign of *Caracalla*, came the general Constitution of that Emperor; the Memory of which being fresh might probably occasion the Insertion of the Words *Civium Romanorum* in this Monument. *In orbe Romano qui sunt, ex Constitutione Imperatoris Antonini Cives Romani effecti sunt*, are the Words of *Ulpian*, in the first Book of the *Digest. Tit. de Statu Hominum*, Law XVII. That the *Antoninus* there mention'd was our *Caracalla*, is abundantly made good by Baron *Spanhem*,

hem, in his Comment upon that Text. To which let me add the Words of *Prudentius*, produced by the ſame learned Writer upon the Occaſion.

Hinc frenaturus rabiem Deus, undique gentes
Inclinare caput docuit ſub gentibus îſdem,
Romanoſque omnes *fieri, quos Rhenus et Iſter,*
Quos Tagus auriſluus, quos magnus inundat Hiberus.
Corniger Heſperidum quos interlabitur, et quos
Ganges alit, tepidique lavant ſeptem oſtia Nili.
Jus fecit commune pares, et nomine eodem
Nexuit, et domitos fraterna in vincla redegit.

What remains EO⊗, I would reſtore EQ⊗, according to the Model of the *Durham* Inſcription produced above:

COH. VARDVL. C. R. EQ. ∞.

For the Difference in Stones is ſo minute in this Particular, that I will venture to pronounce, tho' I never ſaw the Inſcription, it may as well be one as the other. And I read the whole Title of the Cohort thus:

CO*Hors* I*ma* FIDA VARDVL*orum* C*ivium* R*omanorum* EQ*uitatæ* M*illiaria* ANTONINIANA.

Concerning which I have ſomething to offer.

The frequent mention of Equeſtrian Cohorts, or, to ſpeak more adequately, of *Cohortes Equitatæ*, in old Inſcriptions, I obſerve to have been a great

Choque upon ſeveral Antiquaries, who have been taught to conſider the Cohorts as appropriated to the Foot Service, as the *Alæ* and *Turmæ* were to the Horſe. Mr. *Horſley* in particular, *p.* 94. imagines, the *Cohors prima Claudia Equitata*, which he met with in the *Notitia*, was intended to intimate, that this Cohort had been promoted from the Foot to the Horſe Service. But when that Gentleman was led, by the Mark or Monagram in the *Durham* Inſcription referr'd to in theſe Papers, to conſider that Corps as conſiſting of a thouſand Horſe, his Difficulty is increaſed to that Degree that he does not know what to affirm upon it. Now of all this there is a very eaſy Solution.

The Auxiliary or Provincial Cohorts (for of them only I obſerve what follows) were either intirely or purely Foot, like the Legionary and ordinary Cohorts; or elſe they had a Mixture of both Kinds of Militia, and conſiſted of Horſe and Foot together.

IMP. CAESAR. DIVI. VESPASIANI. F. DOMITIA
NVS. AVGVSTVS. GERMANICVS. PONTIFEX. MAXI
MVS. TRIBVNIC. POTEST. XII. IMP. XXII. COS. XVI.
CENSOR. PERPETVVS. P. P.
PEDITIBVS. ET. EQVITIBVS. QVI. MILITANT. IN. COHO
RTE. III. ALPINORVM. *etc.*

Gruter. DLXXIV. 5.

This latter Sort, as they could not properly be rank'd under either Denomination of Horſe or Foot, (for they were made up of both) ſeem to have appropriated to themſelves the diſtinguiſhing Title of COHORTES EQUITATAE, Corps of Infantry with a Mixture

Mixture of Horſe. And of this Term, ſo very ſignificant, and ſo little underſtood, I find frequent Mention.

L. BRVTTIO. L. F.
PAL. CELERI.
EQVO. PVBLIC.
PRAEF. COH. III. AVG.
THRAC. EQVIT.

Gruter. DXXXIV. 2.

Q. CAECILIO. Q. F.
AN. OPTATINO.
PRAEF. COH. I. AQVI
TANOR. EQVIT.
PONTIA. T. F. SABI
NA. MATER.
FLAMINICA.
L. D. D. D.

Gruter. DXXXIV. 4.

P. LICINIO. P. F.
GAL. LICINIANO.
PRAEFECTO.
COHORTIS. VII.
PRAELECTORVM.
EQVITATE. IN.
GERMANIA.
TRIBVNO.
MILITVM. LEG.
VII.
EVIA. PRAEFECTO.

Gruter. DL. 4.

P.

P. LICINIO. P. F.
GAL. MAXIMO.
PRAEFECTO.
COHORTIS. II.
GALLORVM.
EQVITATE. IN.
DACIA. TRIBVN.
MIL. LEG. VII.
CLAVDIAE. PIAE.
FIDELIS. P. LICINIVS.
LICINIANVS.
FRATRI.

Gruter. DL. 5.

M. MAENIO. C. F. COR. AGRIP
PAE. ET. VSIDIO. CAMPESTRI.
HOSPITI. DIVI. HADRIANI.
PATRIS. SENATORIS PRAEF. COH.
II. FL. BRITTON. EQVITAT.
ELECTO. A. DIVO. HADRIANO.
ET. MISSO. IN. EXPEDITIONEM. BRIT
TANNICAM. TRIB. COH. I. HISPA
NOR. EQVITAT. PRAE. ALAE. I.
GALLOR. ET. PANNONIOR. CATA
PHRACTAE. *etc.*

Reines. VI. 128.

Nor have we these Testimonies only, but also a full and decisive Proof of this Denomination, and, what is yet behind, of their Number also, in a Writer very well vers'd in military Affairs, *Hyginus*, who wrote

wrote a Treatise *de Castrametatione*, in the Time of *Trajan*. From him we are informed, that these Troops were call'd *Milliariæ*, as consisting of a thousand private Men, Part Horse and Part Foot. The Proportion of the former of these to the latter was nearly as one to three, *viz.* 240 to 760, instead of 250 to 750. Which little Difference was possibly occasion'd by the Necessity of dividing them into *Centuriæ* and *Turmæ*. The Author's Words are:

Habet Cohors Equitata Milliaria *pedites septingentos sexaginta, centurias decem, equites ducentos quadraginta, turmas decem.*

And again;

Meminerimus itaque ad computationem cohortis equitatæ milliariæ pedaturam ad mille trecentos sexaginta dari debere.

Which is to be thus explained: The *Pedatura* of this irregular Corps, in their Incampment, would not be the same as the *Pedatura* of an uniform Body of Infantry, of the same Number, but would exceed it by 360 Feet; for the Proportion of the Room of one Horseman to one Foot-Soldier he assigns as $2\frac{1}{2}$ to one

Omnis miles provincialis accipit pedaturam, pedem, adjecta quinta——eques autem duos semis, adjecta quinta.—— Habebit itaque Cohors equitata milliaria equites ducentos quadraginta, quos redigo ad peditem, ut pedem, quod accipit miles, ad duos semis, quod accepit eques. Fit dimidia sumpta ductum quinquies: sic tractabimus numerum equitum, fit centum viginti: ducemus quinquies, fiunt sexcenti:

ſexcenti: accedunt ex eadem Cohorte milliaria, detractis equitibus, reliqui pedites ſeptingenti ſexaginta: fit cum ſuperiori mille trecenti ſexaginta.

For the Mark ⊕ I account thus: The uſual Note of a Thouſand is either I between CC's, thus CIↃ; or elſe X, thus CXↃ. The former Figure, when cloſed at the Top, exactly reſembles an ancient M, thus ↀ; and the latter, when ſhut up, a Figure of Eight inclined ∞. Both which Marks have been long uſed to expreſs a Thouſand. The latter is the Mark before us, the X between CCs, but cloſed in on all Sides, thus, ⊕, if this be in Reality the Figure upon the Stone. For Mr. *Gordon* in his *Iter Septentrionale* copying an Inſcription, wherein there was the Mark of four Thouſands, gave us the Thouſand incloſed on all Sides, the very Mark in our Inſcription; but upon Mr. *Horſley*'s Inſpection it turned out to be the ſecond Figure, the Thouſand incloſ'd only at both Ends thus ∞.

The laſt Part therefore of the Inſcription is to be thus underſtood:

COH*ors* I.*ma* FIDA VARDVL*orum Civium Romanorum* EQ*uitata* M*illiaria* ANTONINIANA FECIT SVB CVRA T. CO......... L*egati*, T*ribuni*, or C*enturionis* LEG*ionis* XX.*ma* G*enio* R*omæ*.

Which laſt Words are to be applied to the Emperor, and contain a Compliment at that time of Day not unuſual.

St. John's College, *Cambridge*.
Jan. 1. 1744-5.

IV. *An Abstract of the Rev. Mr.* Gould's *Account of* English Ants; *in a Letter from the Rev* Henry Miles, *D.D. and F.R.S. to Mr.* Henry Baker, *F.R.S.*

Read Jan. 15. 1746-7.

I HERE send you a short Abstract of an ingenious Treatise on *English* Ants, the Perusal of which has entertained and instructed me not a little; and as the very industrious Author has made more Observations than any other Person amongst us appears to have done, and has discover'd several curious Particulars not mention'd by other Writers on the Subject, I thought you would not be displeased to see a brief Account of the Performance, with a few Remarks and Emendations I have taken the Liberty to make.

The Book is intituled, "*An Account of* English *Ants*;" which contains, 1. Their different Species and Mechanism; 2. Their Manner of Government, and a Description of their several Queens: 3. The Production of their Eggs, and Process of the Young: 4. The incessant Labours of the Workers, or common Ants; with many other Curiosities observable in these surprising Insects: By the Rev. *Wm. Gould, A. M.* of *Exeter-College, Oxon. London:* Printed for *A. Millar*, opposite *Katherine-Street* in the *Strand*, MDCCXLVII. in large 12*mo*.

CHAP. I. Contains a Description of Ants in general, their various Sorts, Colour, and Structure of their Parts.

Five Species of Ants have occurred to the Obſervation of our Author. 1. The Hill Ant, vulgarly called the Horſe-Ant. 2. The Jet Ant. 3. The red Ant. 4. The common yellow Ant. 5. The ſmall black Ant.

Having deſcribed the Size and Colour of theſe, he proceeds to deſcribe the Structure and nice Mechaniſm of Ants with great Accuracy; obſerving, that, beſides the *Viſcera*, there is in the Body of Ants a Bag of corroding ſpirituous Liquor, which they can eject to a conſiderable Diſtance at Pleaſure. This Particular has alſo been obſerved by other Writers.

He ſays, he has met with a Ligament in the red Ant, which uniteth the Breaſt and Body, conſiſting of two Lobes ſomewhat round; but in other Ants there appears but one Lobe, which riſes higher, and is broader, than the Lobes in the red. It is this Species of red Ants, which he has obſerved to have a Sting, of the ſame Contexture with that of a Bee, in Miniature: In other Ants he has met with no Sting; but they bite, or make a ſmall Inciſion, with their Saws, ejecting ſome of the afore-mention'd corroding Liquor, *&c.* The red Ants, which are furniſhed with a Sting, he obſerves live more open, *&c.* and are more bold than any of the others; and therefore ſuch a Weapon is ſerviceable to them.

The Jet-Ants, he informs us, have a peculiar diſagreeable Smell, which he imagines may be a great Preſervative to them againſt an Enemy; —— and that the Spirit which all Ants eject is very ſtrong, affecting at a ſmall Diſtance in the ſame manner as Spirits of Hartshorn.

CHAP.

CHAP. II. Treats of their Colonies, Cells, *&c.*

Here our Author obſerves, that tho' they unite in Colonies, in ſuch Places and Situations as are moſt agreeable to their different Natures, *&c.* yet their Reſidence is not ſo limited as to admit no Variation; however 'tis worth obſerving, that the ſeveral Species never ſo intermix, as to aſſociate and breed together, tho' they will live near and good Neighbours one to another.

Their Architecture, he ſays, is adjuſted with remarkable Curioſity and Art, the whole Structure being divided into a Number and Variety of Cells, communicating all of them with one another by little ſubterraneous Chanels, which are circular and ſmooth; but as for the Incruſtation, moſt *Virtuoſi* have mention'd, in the Apartments of Ants, our ingenious Author obſerves, that after the moſt careful Obſervation he could never find any Compoſition in their Structures; the Cells being formed in the Mold itſelf, without any Addition of Glew, Straws, *&c.* He acknowleges it may be otherwiſe in hotter Climates, where Sand is more apt to crumble.

Their Works, as he informs us, are all carried on by the Aſſiſtance of their double Saws, and the Hooks which are placed at the Extremity of them, deſcribed by him in the preceding Chapter. The Proceſs and Manner of their Work may eaſily be obſerved, he ſays, if you depoſit ſome Ants, with a Lump of moiſt Earth under a Glaſs.

CHAP. III. Treats of their Government; deſcribes their ſeveral Queens; the Reſpect ſhewn them by the common Ants, *&c.*

A Colony, out Author tells us, from the latter End of *August* to the Beginning of *June*, is usually composed of a large Female, and various Companies of Workers.——And besides these, in the latter End of *June*, all *July*, and Part of *August*, of a Number of winged Ants commonly known by the Name of Ant-Flies. The Government, he says, has been universally taken for a Republic or Commonwealth; and have been treated as a Body consisting of Males and Females; the former being looked upon to be those which make their Appearance with Wings in the Summer. But as, in the Oeconomy of Bees, the Generality of them have no Distinction of Sex, but make it their whole Employment to provide for the Young laid them by their Queen, so the same Character is found to be maintained in the Constitution of Ants. The common Ants therefore, which usually present themselves to our View, are, he says, like the common Bees, of neither Sex, but seem intirely destined to take care of, and educate the Young, which the Queen deposits in the Cells.

In every perfect Colony, our Author says, there is at least one Queen; who, in the Space of 7 or 8 Months, gives Birth to a Family, amounting, at a moderate Computation, to 4 or 5000; except the red Queens, who are not so prolific. The yellow Ants being the most frequent, he gives a very particular and curious Description of their Queen; which, he tells us, is perhaps 5 times larger than any of her Subjects; and that, moreover, in her Front she has three Eyes, in a triangular Form, which are less than the two common ones on each Side her Head. I omit other Particulars, as also his Description of the other

Queens,

Queens, for Brevity's ſake.---- The Queen of the Jets, he ſays, he never had the Pleaſure of ſeeing.

He has beautifully repreſented the Obedience and Reſpect the Queen commands, in whatever Apartment ſhe condeſcends to be preſent.---An univerſal Gladneſs, he ſays, ſpreads itſelf thro' the whole Cell, expreſſed by particular Acts of Joy and Exultation: They have a particular Way, it ſeems, of skipping, leaping, and ſtanding upon their hind Legs, and prancing with the others; which Frolicks they make uſe of both to congratulate each other when they meet, and to ſhew their Regard for the Queen. Some walk gently over her, others dance around her, and all endeavour to exert their Loyalty and Affection. However romantic, ſays our Author, this Deſcription may ſeem, it may eaſily be proved, by placing a Queen, with her Retinue, under a Glaſs; for, in a few Moments, you will be convinced of the Honour they pay, and Eſteem they have for her.

In *October*, he tells us, Ants and their Queens begin to retire downwards; and, in the Depth of Winter, are to be found in the remoteſt Apartments, incircled cloſe with a Cluſter of Attendants, and, as it were, benumb'd.

CHAP. IV. The Author gives a particular Account of the Time and Manner in which the Queens lay the Eggs, *&c.*

And he ſays, he has been the more circumſtantial in this Point, to remove a Miſtake of Sir *Edmond King*'s, who, not aware of there being a ſuperior Female, gave into the old Opinion, that the

the ſmall Ants were the Females, and ſupplied the Colony with Young: After a juſt Deſcription of the Sperm or Eggs, Sir *Edmond* obſerves, that he found that Subſtance among the common Ants; and that he gave the more Credit to that Opinion, becauſe of the great Care and Tenderneſs with which they treat it.----But our Author does not allow this Reaſon to be concluſive, inaſmuch as the ſame is to be met with in the Conſtitution of Bees; adding, That having at all Times of the Year obſerved the common Ants, he could never diſcern any Alteration in their Bodies but what was occaſion'd by Food, or ſome Accident.

The Queen, he ſays, lays three different Sorts of Eggs, Male, Female, and Neutral: The two firſt in the Spring; the laſt in *July*, and Part of *Auguſt*.

CHAP. V. Our Author treats of the Change of the Eggs to Vermicles, *&c.* and gives us an Account of their ſurpriſing Continuance in that State.

The Queen having furniſhed the Eggs, he ſays, the common Ants brood over them in little Cluſters, perhaps by way of Incubation; and remove them to different Parts of the Colony, for the better Advantage of Moiſture, and a juſt Degree of Heat and Cold. The Time of Continuance in the Egg-State is ſomewhat uncertain: But he ſays they ſeem to diſengage themſelves from the Membranes that incloſe the Eggs in the ſame Manner as Silk-worms do.

The Proceſs of Ant-Vermicles, he tells us, is remarkable, and worth Obſervation. The Female Eggs put on the Form of Worms ſome time in *February*, at fartheſt; the Male by the latter End of

March; the Neutral by *September*. The firſt Summer they grow very ſparingly; the ſucceeding Winter they ſeem at a Stand: In the Beginning of *April* of the ſecond Year they viſibly augment every Day; and in ſix Weeks, or by the End of *May*, the Male and Female attain their greateſt Proportions, and are ready for another Change. This long Continuance of Ants in a vermicular State he thinks a great Curioſity, hardly to be met with in any other Claſs of Inſects---- the Female Ant continuing above a Year and Quarter, the Workers a Twelvemonth, the Males ſomewhat more.

CHAP. VI. Treats of a Tranſmutation of Ant-Vermicles to Nymphs or *Aurelia's*, *&c.*

The Vermicles, he ſays, weave in the Manner of Silk-worms, and in a few Days infold themſelves in a ſoft ſilken kind of Tiſſue: They henceforth aſſume, and, whilſt confined in this Monument, continue the Character of *Aurelia's*. Theſe are the ſmall Bodies which abound in the Settlements in the Summer-Months, and are vulgarly reputed Ant-Eggs; but their Largeneſs, and viſible Tranſmutation (as he juſtly obſerves), ſhew the Miſtake.

Our Author takes notice of a remarkable Variation in the *Aurelia's* of the red Ants. When the Worms arrive at their Period of Tranſmutation, he ſays, they do not infold themſelves in a Tiſſue or Shell, like the others, but lie motionleſs, and, to outward Appearance, inſenſible; in a few Days look whiter than ordinary, and in this manner gradually put on the Form of Ants. Thus Providence (remarks our Author) is tied down

to

to no particular Laws: but can, by a ſurpriſing Variety, accompliſh the ſame Ends.

In the VIIth *Chap.* he proceeds to treat of the Transformation of the ſeveral *Aurelia's* to Flies and common Ants, with a Deſcription of their Structure, Duration, and other Curioſities relating to the Change. But the juſt Progreſs of Ants-Eggs, Vermicles, Nymphs, *&c.* cannot, he ſays, be preciſely ſtated; becauſe they will not arrive at Maturity under Glaſſes, as *Swammerdam*, before him, had obſerved.

As ſoon as the Ant-Nymphs, ſurrounded with a Tiſſue, are tending to Life, he ſays, the Workers give them Air, by an Aperture in the Head-Part of the Covering; which Aperture they gradually enlarge; and, after a Day or two, take out the Young, and expoſe it to the freer Acceſs of the Sun-beams, which are of great Force in promoting its Maturity.

Our Author obſerves, that Philoſophers have uſually confounded the two different Sorts of Ant Flies, the large and ſmall, looking upon them all under the Character of Males; tho' there be ſo wide and manifeſt a Variance in the Colour, Size, *&c.* that the naked Eye may eaſily diſtinguiſh it. —On the contrary, therefore, he preſumes they are of different Sexes: The ſmall ones he takes to be Males, and the large Females; and thinks it highly probable, that ſome of theſe Females, afterwards, give Birth to new Colonies, and intitle themſelves to the Dignity of Queens; there being, as he ſays, many ſtrong Experimental Reaſons to ſupport ſo uncommon a Curioſity; which he alſo recites, and anſwers the chief Objection againſt it, taken from the Number of theſe Ant-Flies: The principal Thing of which his Anſwer conſiſts

consists is, that the most obvious Use of them is for the Sustenance of other Animals.

In the Close of this Chapter he annexes a few remarkable Curiosities resulting from the Change. ---- The casting of their Wings is an Instance, he says, peculiar to the large Ant-Flies; these being to other Insects their highest Decorations; and the Want of them lessens their Beauty, and shortens their Lives. On the reverse, a large Ant-Fly gains by the Loss, and is afterwards promoted to a Throne, and drops those external Ornaments, as Emblems of too much Levity for a Sovereign.

CHAP. VIII. Our Author here treats of the incessant Labours of the Workers, the true Method of collecting their Provisions, and inquires into the Truth of the Opinion of laying up Corn, *&c.* against Winter, *&c.*

He says, The general Subject of this Chapter has been so largely treated of, and well illustrated, by some of the happiest Favourites of *Minerva* and *Apollo*, that it is impossible to set it off with more Beauty of Thought, or Elegance of Stile; but perhaps, (says he) in many Circumstances they have rather shewn the Poet than the Philosopher; and rather indulged an extensive Fancy, than Strictness of Inquiry.

I must here omit the Account the Author gives of the Labour and Industry of the common Ants, which is certainly very curious, that I may avoid being tedious; observing only, in general, that the Feeding the Young is the most laborious Exercise be-

longing to the working Ants, and a Part of their Industry the most uninterrupted of any.

The Juices of most Sorts of Fruit, Insects, and Honey, or any other delicious Liquid, he says, are the Repast which they nurture them with. These Juices they extract, and first convey into their own *Alvus*, and afterwards infuse into the Bodies of the Vermicles; which Aliment, he supposes, may probably undergo some Refinement in the Repositories of the Ants, and, being there meliorated, is properly tempered for the delicate Structure of the Worms.

It has been a Dispute, says our Author, amongst the Inquisitive on this Subject, whether Ants have Magazines of Corn, and lay up a Stock of Provisions against Winter. The Generality of Writers, he says, hold the Affirmative; referring, in his Margin, to *Solomon, Pliny, Virgil, Horace, Aldrovand, Swammerdam, &c.* Here I am obliged to do Justice to *Swammerdam*; who, in his *Biblia Naturæ*, expresly says, that he never at any time observed them to get together any Food against Winter; and is of Opinion, that, during the Severity of the Winter, they eat nothing; as is common with many Insects, and some Species of Bees. His own Words, in *Vol.* I. *p.* 296. are as follow: *Neque etiam unquam observavi, quod cibaria quædam in hyemem sibi comparent: unde censeo ipsas, quousque vehementissima est hyems, nihil comedere; quemadmodum multis insectis, et apum quoque nonnullis speciebus, familiare est; quæ tempore brumali ab omni penitus cibo abstinent.*

Our Author, with great Deference to the Writers who have held the Affirmative, and with extreme Decency, differs from them, offering a handsome

Apology

Apology for himſelf. --- He ſuggeſts, that in warmer Regions they may not undergo the Chill they do with us; and therefore may not paſs the Winter in a State of Numbneſs. ---- That, if this be the Caſe, a Store of Food muſt be neceſſary to them, which is not to our Northern Ants, which live, as it were, entranced. He adds, that, upon the moſt impartial Examination of Authors, the Opinion ſeems rather to be ſupported by its Antiquity, than reduced to a clear Demonſtration. --- He tells us, that, as upon the moſt exact and frequent Examination of numerous Settlements, in the Winter, he could never trace out any Reſervoirs of Corn, or other Aliment; no, not in thoſe of the Hill-Ants, which are the largeſt, and proportionably ſtrong: So, to put this Matter beyond all reaſonable Doubt, he had recourſe to Experiments; which, had the Suppoſition been true, could not probably fail of ſucceeding. At the Beginning of the Spring, he placed, in ſeveral Flower pots, and other Conveniencies, different Colonies of yellow ſmall black Ants, *&c.* with their reſpective Queens, Attendants, and Vermicles; in which Poſition they continued Summer, Autumn, and Winter, and carried on their Operations as in other Settlements, nouriſhed their Young, and brought them to Perfection: From whence he concludes, that they would have laid up Proviſions, had it been their Cuſtom; but, upon carefully examining ſome of theſe Pots, he found no Appearance of Magazines of Corn, or any collected Food: And that, upon his having frequently obſerved their Excurſions from, and Return to their Colonies, he could never find, that they ever return'd with any Wheat, Corn, or any other

 Vegetable

vegetable Seed; tho' they would with Eagerness attack a Pot of Honey, or a Jar of Sweetmeats, *&c.* Many other Experiments, besides these, our Author made, which I forbear to give you, judging these to be satisfactory.

The most material Argument in Favour of Ant-Magazines, he thinks, is the Authority of the Sacred Writings.---- *Solomon,* he says, has twice mention'd these extraordinary Insects; and each time with an immediate Reference to their Sagacity in providing for the Necessities of Winter. — For removing this Difficulty, he has recourse to the former Solution.—— The superior Warmth of the Climate he lived in, and, of consequence, the proportionable Clemency of the Seasons; whence he concludes the Ants of those Countries may vary from ours in this, as well as in other Respects: Or perhaps, adds he, it might have been a received Opinion, as was the Sun's Motion; from whence this great Prince might recommend it, as a worthy Example of Industry and Wisdom.

If I might have Leave humbly to offer my Opinion, our Author seems to have justified his Conduct in departing from the commonly received Opinion: And perhaps there is good Reason to think that it has been handed down from ancient Writers of Reputation, and too easily received, without carefully examining into the Truth of the Fact; which Persons might easily be led to do from a general Observation of the extraordinary Industry of these little laborious Animals in carrying Things into their Cells.

The most learned *Bochart,* in his *Hierozoïcon,* has display'd his vast Reading on this Subject, as he usually

ufually does on all others; and has cited Paffages from *Pliny*, *Lucian*, *Ælian*, *Zoroafter*, *Origen*, *Bafil*, and *Epiphanius*, *Jewifh Rabbi's*, and *Arabians*, all concurring in the Opinion, that Ants cut off the Heads of Grain, to prevent their germinating: But he confeffes, that the ancienter *Greek* Writers have made no fuch Obfervation of the Ants; nor any of them who lived before *Pliny*, as far as he remembers. Very probably this Opinion arofe from what might have been obferved of thefe laborious Infects, in cutting afunder with their Saws fuch Grains of Corn, or other Matters, which they might have Occafion to carry to their Nefts, but were too bulky; for that they cut off Grafs, and other Things, which they find in the Road to and from their Repofitories, our Author has obferved: And it is obfervable, that the *Hebrew* Name of the Ant נמלה *Nemala*, from the Verb נמל, *Namal*, which fignifies to cut off, is ufed for cutting off Ears of Corn (*Job*, xxiv. ver. 24.*)

But if we confider the two Texts, in the Book of *Proverbs*, cited by our Author, there is not the leaft Intimation in them of their laying up Corn in Store *againft Winter*. In *chap.* vi. *ver.* 8. it is faid, *She provideth her meat in the fummer, and gathereth her food in the harveft:* For, tho' the former Verb הכין *Hekin* fignifies to prepare, or difpofe in Order, and the latter אגר, *Agar* to collect, or gather together;

* I might alfo have referred to the *Theatrum univerfale omnium Animalium* of *Jonfton*, publifh'd by Dr. *Ruyfch* junior of *Amfterdam*, in 2 Volumes *folio*, Vol. II. p. 85.

together; and in the only two Places where I find it occur besides, is used for Gathering in Summer, as *Prov.* x. 5. and for Gathering in the Vintage, *Deut.* xxviii. 39. yet the Expressions, in the Text, necessarily mean no more, than that they collect their Food in its proper Season; —— nor is there any thing else declared *chap.* xxx. *ver.* 25. So that all which may fairly be concluded from Scripture is, that they carry Food for themselves into their Repositories *. That they do this against *Winter* can only be determined by examining into the Fact: This our Author has done with very great Diligence, and has discovered, with respect to our *English* Ants, that they eat not at all in the Winter and have no Stores laid in of any sort of Food. The Opinion therefore of their laying in Magazines against Winter, seems to me to have been grafted on these Scriptures, rather than found in them; and this from a Conclusion naturally enough made, from observing (as I said) their wonderful Labour and Industry in gathering their Food in the Summer, —— supposing that this must be to provide against Winter. —— And, after all, great Part of their Labour, which may have been bestowed in other Services, might easily be mistaken, by less accurate Observers, for carrying in Food.

I am sorry I must omit the ingenious Author's just moral Reflections; but my Time will only allow me to conclude, as he does, with the Words of the Royal Psalmist, *Great is the Lord, and marvellous,*

worthy

* *i. e.* To serve them as long as it will keep good, or they shall need it.

worthy to be praised, and there is no End of his Greatness. I am,

Dear Sir,

Tooting, Dec. 10. 1746.

Your most affectionate, and obliged humble Servant,

H. Miles.

V. *A Remark on Father* Hardouin's *Amendment of a Passage in* Pliny's Natural History, *Lib.* II. § LXXIV. *Edit. Paris. folio*, 1723. *by* Martin Folkes, *Esquire, Pr. R. S.*

Read Jan. 22. 1746-7.

VAsaque horoscopa non ubique eadem sunt usui, in trecentis stadiis, aut ut longissime, in quingentis, mutantibus semet umbris solis. Itaque umbilici (quem gnomonem appellant) umbra in Ægypto meridiano tempore, æquinoctii die, paulo plusquam dimidiam gnomonis mensuram efficit. In urbe Roma nona pars gnomonis deest umbræ. In oppido Ancone superest quinta. Decima in parte Italiæ, quæ Venetia appellatur, eisdem horis umbra gnomoni par fit.

The geographical Reader cannot but observe here immediately, that somewhat is faulty in this Passage as it stands; since the equinoctial Shadow of the *Gnomon* being made shorter at *Ancona* than at *Rome*, the Latitude of *Ancona* will consequently be made lesser than that of *Rome*; whereas it is known to be

be conſiderably greater; *Ancona* ſtanding on the *Adriatic*, about two Degrees to the North of that Capital.

But, upon turning to Father *Hardouin*'s Obſervations upon this Paſſage, I find the Text to have been altered by him in a very remarkable Manner, from all the former printed Editions. His Obſervation is as follows:

Hactenus editum eſt, in oppido Ancone ſupereſt quinta XXX. In parte Italiæ, &c. *MSS. hic variant. Nos ex certiſſima conjectura edi curavimus*, Ancone ſupereſt quinta. *decima in parte Italiæ*, &c. *In decima regione Italiæ Venetiam ſtatuit in geographicis libro ſequente. Neque ſimile veri eſt, in tot particulas, hoc eſt, in quintas triceſimas, ab homine* πλαſ]ικωτέρως *ubique agente, gnomonem umbramve dividi.*

Upon which Words it may be noted, that altho' the Reverend Father acquaints us he had met with ſome Variation in the Manuſcripts, yet he appeals to none; nay, he even tells us expreſly, that his Amendment was purely made upon Conjecture; whence we may ſafely conclude, that it ſtands unſupported by any various Reading or Authority whatſoever. He at the ſame time alſo acknowleges, that all the printed Editions conſpire in another Reading; which I have found to be true in ſeveral I have had Occaſion to look into, with this only Variation, that whereas the firſt Edition in 1469, and ſeveral of the following ones, print the Word *quinta* at Length, and XXX only in Figures; ſome of the later ones, and that by the *Elzevirs* particularly in 1635, print both the Words at Length; the whole Paſſage running thus:

Umbilici (quem gnomonem appellant) umbra in Ægypto meridiano tempore, æquinoctii die, paulo plus

plus quam dimidiam gnomonis mensuram efficit: in urbe Roma nona pars gnomonis deest umbræ: in oppido Ancone superest quinta trigesima: in parte Italiæ quæ Venetia appellatur, iisdem horis umbra gnomoni par fit.

The plain Meaning of these Words is only this, that the Length of the Shadow of a *Gnomon* or upright Style at Noon, on the Day of Equinox, is, in *Egypt*, little more than half the Height of the *Gnomon*; that the same at *Rome* wants a ninth Part of that Height; that at *Ancona* the Height of the *Gnomon* exceeds the Length of its Shadow, by one thirty-fifth Part, or is in Proportion to it as 35 is to 34; and that, in the Part of *Italy* which is called *Venetia*, the Length of the Shadow, and the Height of the *Gnomon*, are equal to each other.

The Particulars here mention'd are respectively true, in the 4 following Latitudes, 26° 34′, 41° 38′, 44° 10′, and 45°. The first of which is the Latitude of the middle Parts of *Egypt*, and the last that of several Places in the Territories of *Venice*, the City itself standing, according to *Manfredi's* Table, in the Latitude of 45° 33′, and *Padua* in that of 45° 28′. The Latitude of this last Place is given by *Ptolomy* 44° 30′, and that of *Aquileia* at the Head of the *Adriatic* in the *Friuli* exactly 45 Degrees.

The Latitude of *Rome*, according to *Ptolemy* 41° 40′, only exceeds that collected from the Text before us by 2 Minutes; and his last again falls short 15′$\frac{1}{2}$ of that delivered by *Manfredi* and *Bianchini* 41° 54′$\frac{1}{2}$. Besides which it may be noted, that the very Fact here mention'd is also spoken of by *Vitruvius*, as Father *Hardouin* has himself in another of

his Notes observed. *Sol æquinoctiali tempore ariete libraque versando, quas ex gnomone partes habet novem, eas umbræ facit octo, in declinatione cæli, quæ est Romæ.*

We come now to the Latitude of *Ancona*, which is given by *Manfredi* 43° 54′, or 16′ less than that above collected from *Pliny*; but which is set down by *Ptolemy* 43° 40′, half a Degree less than the same. This may however be looked upon as no bad Observation, considering the Time when, and the Manner how, it is supposed to have been made, as we are ignorant of the Hour when the Sun really crossed the *Equator* on the Days of Observation; and especially as it comes, with all its Imperfections, as near to the Truth, as that reported both by our Author and *Vitruvius* to have been made at *Rome* itself; and as it only exceeds the true Latitude by about the same Quantity, which that given by *Ptolemy* seventy or eighty Years afterwards fell short of the same.

It therefore appears, upon the whole, that this Text needed no Correction; and for the Observation, that 35 Parts were too many for a *Gnomon* to be divided into, it will be found to have very little Weight, when it is considered, that the Antients made use of very large *Gnomons* upon these Occasions; that one of the Obelisks now standing at *Rome*, that of St. *John's Lateran*, is in Height 108 *English* Feet without the Pedestal; and that the other, still buried under the *Campo Marzo*, which was formerly used for this very Purpose, wanted but little of the same Height. The thirty-fifth Part therefore of the Height of such a Stone, did not fall

short

ſhort of three *Engliſh* Feet; a much leſs Quantity than which would eaſily diſcover itſelf in the Shadow, whoſe Length, notwithſtanding all Difficulties ariſing from the *Penumbra*, might certainly be determined to leſs than half a Foot.

I ſhall juſt add to this Remark the Deſcription given by *Pliny* of this *Gnomon*; who, ſpeaking in his xxxvi. *Book*, § XIV. of the Obelisks that were at *Rome* in his Time, adds, in the Beginning of § XV. *Ei, qui eſt in campo, divus Auguſtus addidit mirabilem uſum, ad deprehendendas ſolis umbras, dierumque ac noctium ita magnitudines, ſtrato lapide ad magnitudinem obeliſci, cui par fieret umbra, brumæ confectæ die, ſexta hora, paulatimque per regulas (quæ ſunt ex ære incluſæ) ſingulis diebus decreſceret, ac rurſus augeſceret.* From which Deſcription I underſtand, that there was laid down, from the Foot of the Obeliſc Northward, a level Pavement of Stone, equal in Breadth to the Breadth of the Obeliſc itſelf, and equal in Length to its Shadow at Noon upon the ſhorteſt Day; that is to ſay, that its Length was to the Height of the Obeliſc almoſt as 22 are to 10; and that into this Pavement there were properly let in parallel Rulers of Braſs, whoſe Diſtances from the Point, directly under the *Apex* of the Obeliſc, were reſpectively equal to the Lengths of the Shadow thereof at Noon, on the ſeveral Days of the Year; as the ſame Lengths decreaſed from the ſhorteſt Day to the longeſt, and again increaſed from the longeſt Day to the ſhorteſt.

After which the Author mentions in a Paſſage greatly corrupted, and therefore now almoſt unintelligible; that one *Manilius*, or *Manlius*, had added

to the Top of the Obelisc a gilded Ball, whose Use was to make the Shadow of the Extremity the more observable, as the middle Part of the Shadow of that Globe could readily be estimated; whereas the Shadow of an *Apex* would, at so great a Distance, be intirely imperceptible.

VI. *A Letter from the Rev. Mr.* Mason, Woodwardian *Professor at* Cambridge, *and F. R. S. to the* Pr. R. S. *concerning* Spelter, *Melting* Iron *with Pit-coal, and a* burning Well *at* Broseley.

SIR,

Read Jan. 22. 1746 7.

HAving met with several Things, in a Ramble last Summer, that were new to me, and imagining they might be so to you likewise, and being of some Consequence, I presume to trouble you with a short Account of some of them.

What *Spelter* is I don't well know, nor what Uses are already made of it; but I believe it was never yet applied to so large a Work as the Cylinder of a Fire-Engine, till Mr. *Ford*, of *Colebrook Dale* in *Shropshire*, did it with Success: It run easier, and cast as true as Brass, and bored full as well, or better, when it had been warmed a little: While cold, it is as brittle as Glass, but the Warmth of my Hand soon made it so pliant, that I could wrap a Shaving of it round my Finger like a Bit of Paper. This Metal

tal never rusts, and therefore works better than Iron; the Rust of which, upon the least Intermission of working, resists the Motion of the *Piston*.

Several Attempts have been made to run *Iron Ore* with Pit-coal; I imagine it hath not succeeded any-where, because we have had no Account of its being practised; but I find that Mr. *Ford*, from Iron Ore and Coal, both got in the same Dale, makes Iron brittle or tough, as he pleases; there being Cannon thus cast so soft as to bear Turning like wrought Iron.

At *Broseley*, about a Mile from the fore-mention'd Place, in the Year 1711, was a Well found, which burned with great Violence, whereof some Account is given in *Philos. Transact.* N°. 334; but it has been many Years lost. The poor Man, in whose Land it was, missing the Profit he used to have by shewing it, applied his utmost Endeavours to recover it; but all in vain, till *May* last; when, attending to a rumbling Noise under the Ground, like what the former Well made, tho' in a lower Situation, and about 30 Yards nearer to the River, he happen'd to hit upon it again.

That you may have some Notion of what it is, I will lay before you such an Account of it, as the cursory View I had will permit.

The Well for 4 or 5 Feet deep is 6 or 7 Feet wide; within that is another less Hole, of like Depth, dug in the Clay; in the Bottom whereof is placed a cylindric earthen Vessel, of about 4 or 5 Inches Diameter at the Mouth, having the Bottom taken off, and the Sides well-fix'd in the Clay ramm'd close about

it.

it. Within the Pot is a brown Water, thick as Puddle, continually forced up with a violent Motion, beyond that of boiling Water, and a rumbling hollow Noise, rising and falling by Fits 5 or 6 Inches; but there was no Appearance of any Vapour rising; which perhaps might have been visible, had not the Sun shone so bright.

Upon putting down a Candle at the End of a Stick, at about a Quarter of a Yard Distance, it took Fire, darting and flashing in a violent Manner, for about half a Yard high, much in the manner of Spirits in a Lamp, but with a greater Agitation. The Man said, that a Tea kettle had been made to boil in 9 Minutes Time; and that he had left it burning 48 Hours together, without any sensible Diminution.

It was extinguished by putting a wet Mop upon it, which must be kept there a small time; otherwise it would not go out. Upon the Removal of the Mop, there succeeded a sulphureous Smoke, lasting about a Minute; and yet the Water was very cold to the Touch.

The Well lies about 30 Yards from the *Severn*; which, in that Place, and for some Miles above and below, runs in a Vale full 100 Yards perpendicular below the Level of the Country on either Side, which inclines down to the Country at an Angle of 20 or 30 Degrees from the Horizon; but somewhat more or less in different Places, according as the Place is more or less rocky.

The Country consists of Rock, Stone, Earth, and Clay, unequally mix'd; and as the River, which is very rapid, washes away the soft and loose Parts, the next successively slip into the Chanel; so as, by degrees,

degrees, and in time, to affect the whole Slope of the Land: And as the inferior *Strata* yield Coal and Iron-Ore, their Fermentation may produce this Vapour, and force it to ascend with Violence through the Chinks of the Earth, and give the Water the great Motion it has. This might be obstructed in one Place by the forementioned subsiding of the sloping Bank, and might afterwards find a Vent in another; in like manner as it happen'd at *Scarborough Spaw*, a few Years since.

If these Hints should be any Amusement to you, or be the Means of setting any more able Person upon further Inquiries, and giving a better Account of them, I have all that is intended by

Jan. 18. 1746. *Your humble Servant,*

Cha. Mason.

VII. *Part of a Letter from Mr.* John Browning, *of* Bristol, *to Mr.* Henry Baker, *F.R.S. dated* Dec. 11. 1746. *concerning the Effect of* Electricity *on* Vegetables.

Read Jan. 22. 1746-7.

HAving an Operator at *Bristol* with a good electrifying Machine, I was desirous to electrise a Tree, and therefore sent him the following for that Purpose; *viz. Laurustinus*, *Leucoium majus flore pleno ferrugineo*, and *Stœchas citrina Cretica*. These were not chosen with any

Deſign; their being the leaſt Plants I had, was the only Reaſon.

I promiſed myſelf the Pleaſure of ſeeing their Leaves erected when electriſed, but was diſappointed (whether its being the dormant Seaſon of the Year for all Plants might not be ſome Hindrance, I cannot determine); neither did the Leaves flag on their being touched. However, I was agreeably recompenſed, by a Stream of fine purple blue coloured Light, much reſembling an Amethyſt, that iſſued from the Extremity of each Leaf upwards, of an Inch in Length, when the Finger, or any other Non-electric, approached near it. This Colour I attribute to the watry Particles in the Earth, having often obſerved the very ſame Colour iſſuing from the long Leg of a Syphon. On putting my Finger on the Gun-barrel to ſtop the Electricity, the Leaves of each Tree had a trembling Motion, which remained for ſome little Time, and immediately ceaſed on withdrawing my Finger from the Barrel, and admitting the Electricity. This conſtantly happened, as I put my Finger on or off the Barrel.

The *Stœchas* Plant has a very long hoary Leaf, and bears its Bloſſom on a very ſmall, ſlender, and almoſt naked Stem, riſing near a Foot above the Body of the Plant. This Stem had a Motion given it, when any Non-electric was brought within about two Inches of its Summit, much like the Vibration of the *Pendulum* of a Clock; which vibrating Motion was parallel with the Breech of the Gun, quite contrary to the ſame kind of Motion I had before obſerved in a Needle, hanging perpendicularly by a Thread at the End of the Gun; the Needle always vibrating

vibrating in the Direction of the Gun. The Motion of the Plant and Needle always continued as long as the glass Globe was excited.

I was also desirous to be satisfied, whether Electricity could be propagated without mutual Contact, by suspending another Gun in Silk Cords, about two Inches from Contact, and the Electricity was near as strong in the second Gun as in the first. At the Distance of between 3 and 4 Inches it was much abated, and so it gradually diminished, as the Distance increased to near 6 Inches, where it would scarce attract a Thread of Trial.

I prevailed on a Man to be let Blood, and then placed him on a Cake of Pitch, but could not be sensible of any Increase of Velocity in his Blood, by being electrized, as has been asserted.

I had almost forgot to mention, that the Strokes I received from the electrified Garden-Pots were more violent and painful to my Fingers than from any other Body I ever experienced.

Mr. *Baker*, since his receiving the above Account, has had an Opportunity of electrifying a Myrtle-tree, of between 2 or 3 Feet in Height, growing in a Pot, at the Seat of the Duke of *Montague* at *Ditton*; in Presence of His Grace, of the *President* of this *Royal Society*, and several other curious Gentlemen; who found, that whenever the Hand, or other non-electric Body, was brought near the Leaves, Streams of fine purple Fire issued therefrom, together with a considerably cold Air; and that the Leaves would be attracted at some Distance, and move vigorously towards a non-electric Body.

VIII. *Mercurius* ſub Sole viſus in Specula aſtronomica Academiæ *Gieſenſis*, Anno 1743. die 5 *Nov.* a *Christiano Ludovico Gerſten*, Math. Prof. et R. S. *Lond.* Sod.

Read Jan. 22. 1746-7.

IN obſervatione hujus tranſitus tribus potiſſimum machinis uſus ſum, quarum brevem deſcriptionem ut præmittam, inſtituti ratio ante omnia poſtulabit.

Prima atque præcipua erat tubus aſtronomicus 10 pedum bonæ notæ, cui aptavi micrometrum exquiſiti operis *Londini* comparatum. Præcipua ſtructuræ in eo conſiſtunt. In centro tubi loco convenienti ad angulos rectos ſeſe decuſſant duo fila immobilia, et alia rurſus duo, ſitu ad unum mobilium parallelo per motum unius cylindri chalybei duplici cochlea (quarum una ad dextram, altera ad lævam vergit) æqualibus helicibus ſtriati, continue et æqualiter pro diverſo cochleæ motu aut verſus centrum progrediuntur, aut ipſum poſt ſe relinquunt, addita machinula peculiari duobus indicibus inſtructa, quorum unus numerum revolutionum integrarum cochleæ, alter numerum diviſionum unius revolutionis monſtrat. Hoc omne rurſus per rotam dentatam et cochleam aliam perpetuam, ubi opus eſt lento, et exacto motu circumagi, atque ad quoſvis angulos et circulos cœli aptari poteſt. Hoc micrometrum ea Caſſiniana methodo, quæ uſitatior eſt ad tranſitum obſervandum, applicavi. Radebat nempe Mercurius filum immobile ad æquatorem parallelum, dum ex mobilibus unum ſolis marginem inferiorem attingebat. Machinæ parallacticæ apparatu

paratu facile carui; nam adducto semel Mercurio ad centrum tubi, rotæ dentatæ atque cochleæ perpetuæ artificio, brevi mora filum immobile in eum situm redigebatur, ut Mercurius super istud incederet.

Secundum instrumentum erat horologium oscillatorium astronomicum, *Londini* ab egregio hoc in genere artifice *Joh. Ellicott*, R. S. Sod. confectum; de cujus exacto iatis, et ad medium motum solis accommodato motu, complurium annorum observationes nullum mihi dubitandi locum relinquunt. Ope hujus automati appulsuum momenta tam Mercurii quam limborum solis ad filum horarium adnotavi.

Tertia machina ad tempus verum meridiei inveniendum inserviebat. Ob defectum quadrantis astronomici eam ad usus meos inveni atque effeci, prolixiorem descriptionem alia fortassis occasione daturus. Quæ vero præcipua sunt, huc redeunt: ut ejus ope momenta temporis complurium duarum æqualium altitudinum solis ante et post meridiem ad 2, 4, summum 6 minutorum secundorum certitudinem (pro ratione nimirum obliquitatis arcus, quem sol transcurrit motu diurno ad lineam horizontalem) determinare atque exinde meridiei verum tempus ad 1, 2, summum 4 sec. certitudinem indagare possum. Et hac quidem ipsa quinta die *Nov.* per duas bonas cum hoc instrumento confectas observationes, quibus sequenti sexto hujus mensis die, alias tres addere contigit, horologii tempus ita correxi, ut citra 4 vel maxime 5 minutorum secundorum errorem, recte istud factum fuisse existimem. Præmissis hisce, ad phænomena me converto.

Paulo post horam matutinam 8, nubes quibus totum coelum foede obductum erat, præter expectationem

dehiſcere cœperunt, atque brevi intervallo per hiatum ſatis magnum ſol apparebat puriſſimo lumine fulgens. Tubo ſtatim applicato cum nihil adhuc de Mercurio, nullaque prorſus macula in eo conſpiciebatur, id egi, ut diametrum ejus horizontalem repetitis compluribus obſervationibus capeſſerem, quod etſi ob motus rapiditatem non ita facile fuerit factu, exiſtimavi tamen non male tandem deprehenſam fuiſſe ſemidiametrum ejus micrometri rev. 21. et $\frac{22}{72}$. Verticalem ſemidiametrum poſtea ad hor. 11. min. 20. circiter reperi exacte 21. rev. $\frac{18}{72}$. Quantum hi numeri in partibus circuli maximi efficiant, iſtud infra indicabitur.

Capta ſemidiametro ſolis horizontali, denſiſſimæ rurſus nubes eum occultabant; ſed hor. 9. min. 6. ſec. 25. ſubito Mercurium in ejus diſco conſpexi, jam totum, ſi recte memini, ingreſſum in margine tamen adhuc hærentem. Forte nomenclator, qui horologii oſcillatorii numeros viva voce mihi indicaret, aberat: quapropter ſtatim ab hoc ſpectaculo ad horologium temporis, notitiam ut caperem, evolavi, in reditu reliqua obſervaturus. Nam ultra 5 paſſus non diſtabat, ſed eo ſitu, ut a tubo ad indices non pateret proſpectus; aſt ſcriptis in ſchedam numeris tectum nubibus ſolem reverſus inveni: inde factum ut non audeam affirmare, quod exacte tunc temporis in contactu marginis ſolaris conſtitutus fuerit.

Quæ nunc ſequuntur obſervationes, nubium intervalla conceſſêre. Favebat tamen aëris tranquillus ſtatus, favebat et abſentia multorum ſpectatorum. Corpuſculum Mercurii rotunde nigrum margine determinata abſque ullis atmoſpheræ veſtigiis apparebat, ſed tantæ exilitatis, ut quantum nudo oculorum judicio

dicio conjectare licuit, non multum ultra duplum crassitiei fili immobilis in micrometro assurgeret, quod ex pilo capitis humani constabat. Circa hor. 1. post mer. min. 10. usque ad egressum sat magno intervallo nubes patebant; sed purior aër atque longior solis mora in eo, circa parietem observatorii, indirectum tunc ferme soli obversum calorem effecit: inde trepidatio atque undulatio limbi admodum importuna quam nullis remediis amovere potui.

Prima sequentis tabulæ columna exhibet tempus horologii. Secunda tempus verum correctum. Tertia, intervalla temporum, ab appulsu limborum solis ad appulsum Mercurii ad filum horarium, reducta atque conversa in minuta secunda circuli maximi, pro declinatione solis 15 gr. 39 min. 18 sec. Quarta, observationes. Quinta, distantias Mercurii a limbo inferiori solis, in partibus micrometri. Sexta denique, micrometri partes ad scrupula secunda circuli maximi reductas. Basis reductionis est: 23 revolutiones integræ dant $17' \; 33''\frac{1}{2}$. Quam ex solis et fixarum nonnullarum transitu talem inveni.

Temp.

Temp. hor. ante merid.	Temp. verum correctum.	Dist. Merc. in A.R.	Observatione.	Rev. micr.	Dist. ☿ in Decl.
9 6 25	9 5 55	00	1. ☿ tot. ingress. vidi aut certe quoad maximam partem.		
9 44 7 0 0 55	9 43 37 0 44 25	694	2. ☿ ad horarium limb. solis sequens ad horar.		
9 49 5 0 0 55	9 48 35 0 49 25	722	3. ☿ ad horarium limb. solis seq. ad horar.		
9 56 30 0 57 23	9 56 0 0 0 53	766	4. ☿ ad horarium limb. solis seq. ad horarium per nubeculam tenuem obs.		
10 2 43 0 3 38	10 2 13 0 3 8	794	5. ☿ ad horarium limb. solis seq. ad horar.		
10 44 35 0 45 44	10 44 5 0 45 14	996	6. ☿ ad horar. limb. solis seq. ad horar.		
10 48 14 0 49 25	10 47 44 0 48 55	1028	7. ☿ ad horar. limb. solis seq. ad horar.		
10 53 26 0 54 39	10 52 55 0 54 9	1054	8. ☿ ad horar. limb. solis seq. ad horar.		
11 33 52 0 35 20	11 33 22 0 34 50	1272	9. ☿ ad horar. limb. solis seq ad horar.		
11 40 54 0 42 25	11 40 24 0 41 55	1310	10. ☿ ad horar. limb. solis seq. ad horar.		
11 43 52 0 44 35 0 46 7	11 43 22 0 44 5 0 45 37	621 1329	11. Limb. solis præced. ad hor. ☿ ad horarium. Limb. solis seq. ad horar.		

Tempus horologii.	Temp. verum correctum.	Dist. Merc. in A. R.	Observationes.	Rev. micr.	t. ☿ in ☉ cl.
A. M.	P. M.				
h ′ ″	h ′ ″		12.		″
11 57 42	11 57 12		Limb. solis præced. ad hor.		
0 58 20	0 57 50	549	☿ ad horarium.	$12\frac{4}{7}\frac{5}{2}$	580
0 59 57	0 59 27	1401	Limb. solis seq. ad hor.		
P. M.			13.		
12 3 27	12 2 57	520	Limb. solis præced. ad horar		
0 4 3	0 3 33		☿ ad horar.	$13\frac{\,}{7}\frac{8}{2}$	600
0 5 42	0 5 12	1430	Limb. solis seq. ad horar.		
			14.		
12 46 56	12 46 26	289	Limb. solis præced. ad horar		
0 47 16	0 46 46		☿ ad horar.	$15\frac{4}{7}\frac{4}{2}$	715
0 49 11	0 48 41	1661	Limb. solis seq. ad horar.		
			15.		
1 16 56	1 16 28		Limb. solis præced. ad horar		
0 17 8	0 0 38	144	☿ ad horar.	$17\frac{1}{7}\frac{6}{2}$	788
			Limbus solis trepidare cœpit		
			16.		
1 19 27	1 18 57		Limb. solis præced. ad horar		
0 0 35	0 19 5		☿ ad horar.	$17\frac{2}{7}\frac{4}{2}$	793
0 21 41	0 21 11		Limb. solis seq. ad horar.		
			17.		
1 36 15	1 35 45		Marginem int. tangere vide		
0 0 20	0 35 50		batur, certe tetigit aut paulisp		
0 38 19	0 37 49		superavit, penitus in limbo		
			evanuit ingens undulatio ac		
			trepidatio limbi		
			Obs. 16.		
			Ob trepiditationem limbi solarias ad analog. reliquorum sic corrigendam duco.		
1 19 $26\frac{1}{2}$	1 18 $56\frac{1}{2}$	122	Limb. solis præc. ad horar.		
0 0 35	0 19 5	1828	☿ ad horar.	$17\frac{2}{7}\frac{4}{2}$	793
0 21 $41\frac{1}{2}$	0 21 $11\frac{1}{2}$		Limb. solis seq. ad hor.		

Nunc

Nunc ad corollaria progredior ex his observationibus derivanda. Ante omnia diameter solis determinanda: ut id rite fiat, opus, est ut habeamus declinationem, necnon altitudinem ejus eo tempore quo diameter verticalis mensurata fuit. Declinatio solis facile ex longitudine ejus supputatur. Longitudinem ex tabulis *Ludovicianis* ad hor. 11. 20′ 39″ temporis veri (medium scilicet propemodum transitus) pro meridiano 25′. 10″. temporis a *Parisiensi* versus orientem distantis deprehendi ♏ 12°. 37′. 37″. Huic longitudini respondet declinatio australis 15°. 39′. 18″. Intervallum temporis ab appulsu limbi solis præcedentis ad appulsum sequentis per obs. 11, 12, 13, et 14, est 2′. 15″. Quod tempus in arcus æquatoris conversum dat 33′. 45″. Quodsi igitur hic arcus pro declinatione inventa secundum sphæricæ doctrinæ regulas ad partes circuli maximi reducatur, prodit solis diameter 32′. 30″.

Porro ex observationibus astronomicis *Philippi Butisbacensis* Hassiæ *Landgravii* latitudo urbis *Butisbaci* ultra 4 horas itineris a *Giesa* non distantis est 50°. 28′. Quare pro latitudine *Giesensi* accipio 50°. 30′. inde altitudo solis tunc temporis quo diameter ejus verticalis mensurata fuit, est præter-propter inter 23 et 24 gradum. Semidiamer solis in partibus micrometri erat 21 rev. et $\frac{4}{1}\frac{8}{2}$ quod secundum tabulam meam = 16′. 13″. circuli maximi. Inde diameter verticalis eo tempore 32′. 26″. Sed propter refractionem ea justo minor appareri debuit, proditque ex tab. v. *Hireana* defectus iste 4″. addito hoc habemus rursus 32′. 30″. Sin adhibeamus tabulam refractionum recentiorem ex hypothesibus *Taylloriianis* constructam, quam publicavit celeberrimus

Halleius,

Halleius *, atque reliquis præfert; defectus iste nonnulla scrupula tertia tantummodo prodibit minor.

Horizontalem semidiametrum ut supra dixi deprehendi in partibus micrometri 21 rev. $\frac{22}{72}$. Duplum hujus quantitatis † dat secundum tabulam meam 32′. 31″. circ. max. Ergo hæ tres observationes satis bene inter se consentiunt, atque diametrum solis constituunt 32′. 30″.

Pergo nunc ad angulum visum semitæ apparentis Mercurii cum ecliptica. Extra controversiæ aleam hoc positum existimo, quod Angelus sit oporteat, isque divinis machinis instructus, qui complures ejusmodi observationes ita perficeret, ut omnia Mercurii loca per easdem determinata exacte in unam eandemque lineam coincidant. Nam ut taceam refractiones atque parallaxes, quas methodus adhibita non excludit, error semiscrupuli secundi temporis, 7 propemodum minuta secunda in distantia Mercurii a limbo solis sequenti vel præcedenti efficit. Modum igitur quem ob eandem rationem celeberr. *Manfredi* in transitu anni 1736 §, necnon alii se adhibuisse testantur, elegi. Exacta nimirum scala summaque cura omnes in typum retuli; quo pacto deprehendi: quodsi inter loca obs. 15 et 16 correctæ arithmeticé medius locus quæratur, deinde per hunc, necnon per eum quem obs.

* *Phil. Trans.* N°. 368. Vide *The Abridgment* by Mr. *Reid* and *Gray*, Vol. VI. *p.* 160.

† Nam per revolutionem unius cylindri duo fila mobilia simul aut centrum appropinquabant, aut ab eo discedebant, sicuti supra in descriptione micrometri indicatur. Ergo numeri machinæ non diametrum, sed semidiametrum indicabant.

§ *Phil. Trans.* N°. 446, *p.* 106.

obſ. 5 determinat, recta ducatur eam quam proxime veram ſemitam apparentem in diſco ſolis exhibere. Hoc principio poſito numeros adduxi. Diſtantia media inter obſ. 15 et 16 correct. a limbo ſolis ſequenti eſt 1817″ circ. max. Diſtantia media Mercurii inter eaſdem obſervationes a limbo inferiori ſolis 790″½. Diſtantia loco obſ. 5 a limbo ſequenti eſt 794″. Diſtantia a limbo inferiori 288″. Ergo differentia inter diſtantias a limbo ſeq. 1023″. Differentia inter diſtantias a limbo inferiori = 502″½. Hæ differentiæ ergo triangulum rectangulum conſtituunt, quarum prior pro baſi altera pro catheto habenda. Ducto igitur calculo angulus ad baſin provenit 26°. 9′. cui angulus ſemitæ cum circulo ad æquatorem parallelo æqualis. Eodem modo angulos ſingulorum locorum ab obſ. 7. incipiendo cum loco obſervationis 5. quæſivi, et proſiluere ſequentes.

					°	′
Ex obſ.	5	et	7	angulus eſt	26	33
	5	et	8	—	26	33
	5	et	9	—	26	5
	5	et	10	—	26	51
	5	et	11	—	26	21
	5	et	12	—	25	41
	5	et	13	—	26	7
	5	et	14	—	26	13
	5	et	15	—	26	17
				Medium	26	11

Quare cum priori caſu angulus ad baſin ſit 26°. 9′. hoc vero 26°. 11′. medium ſc. 26°. 10′. pro an-

gulo

gulo viso semitæ cum circulo parallelo accipio. Erit ergo ang. semitæ apparentis cum horario 116°. 10′. Sed ad locum solis 12°. 37′. ♏ respondet per. tab. *Hircanam* ang. eclipticæ cum meridiano 107°. 43′. Ergo ang. semitæ apparentis Mercurii cum ecliptica 8°. 26′.

Pro distantia centrorum minima elegi duas observationes, inter quas mediam viam tenere semitam typus indicabat, nec tamen multum ab ipsa semita distabant, septimam nempe et decimam. A distantiis Mercurii a limbo inferiori subduxi 8″, tribuendo $5\frac{1}{2}$″ pro semidiametro Mercurii *, reliquum vero dimidio crassitiei fili paralleli. Nam ex constructione micrometri, in introductione exposita, claret, distantias a centro tubi, non vero a margine fili, assumendas esse: deinde ex distantia Mercurii a limbo sequente in obs. 7. semidiametro solis, angulo invento semitæ cum circulo parallelo, per triangulorum analysin, inveni semitæ sive centrorum solis et Mercurii distantiam minimam 9′. 2″. ex observ. 10. vero simili calculo eadem distantia prodit 9′. 7″. medium ergo, 9′. $4\frac{1}{2}$″. assumo pro vera distantia semitæ a centro solis. His præmissis trigonometrico calculo deduxi sequentia.

		′	″
Semitæ visæ longitudo in disco solis,	——	26	57
Latit. visa Mercurii in conjunctione,	——	9	10
Latit. Mercurii in ingressu ——	——	10	57

Latit.

* Diametrum Mercurii per micrometrum ad tubum *Hugenianum* 120 pedum applicatum, determinavit celeberr. *Bradley*, anno 1723. 10″ 45‴. Vide Phil. *Trans.* N°. 386, *p.* 229.

	′	″
Latit. Mercurii in egressu, — —	6	59
Different. inter latit. in egressu et ingressu	3	58
Portio semitæ inter ☌ et medium transitus,	1	20

Quæ restant, tempus nempe conjunctionis, positio nodi, atque inclinatio orbitæ, ex his quidem immediate non eruuntur; nam requiritur adhuc exacta determinatio moræ centri Mercurii in disco solis, quam ex meis observationibus tuto determinare nequeo. Comparatis tamen intervallis temporum cum distantiis complurium locorum in semita, inveni satis consentientem motum horarium 5′. 56″. adeoque omnem centri Mercurii in disco moram quamproxime accedere ad horas 4. min. 33. Cumque 1 vel 2 minutorum temporis, hac in re error exiguam in nodo imo in orbitæ inclinatione efficiat differentiam, breviter ea subjungere libet, quænam ex hoc hypothetico calculo provenere.

Ponamus igitur, quoniam trepidatio limbi verosimili ratione, contactum Mercurii ad interiorem marginem, adeoque et egressum anticipavit.

	h	′	″
Tempus verum egressus centri ☿ in disco solis *Giesæ*, — — —	1	37	0
Dimidiam moram in disco —	2	16	30
Erit medium transitus *Nov.* 4.	23	20	30
Ex motu horario, et portione semitæ inter ☌ et medium transitus erit tempus per istam portionem —	0	13	28
Ergo temp. ver. conjunct. *Giesæ*, *Nov.* 4.	23	7	2
Ponamus porro differentiam meridianorum inter *Giesam* et obser. *Parisinum*, rejectis secundis — —	0.	45	9

Erit

h ′ ″

Erit tempus verum conjunctionis in obſervat. *Pariſin.* —— —— 22 42 2

Æquatio temporis ex tab. *Ludov.* ſub. 0 20 24

Erit temp. med. conjunct. in obſ. *Pariſ.* 22 21 38

° ′ ″

Ad hoc tempus locus ſolis ex tab. *Ludovic.* —— —— ♏ 12 37 0

Porro ex differentia inter latit. in ingreſſu, et latit. in egreſſu, necnon latit. in ☌ atque mora centri in diſco, ſc. h. 4. m. 33. reſultat tempus quod ☿ abſolvit a ☌ ad ☊ —— —— h ′ ″ 10 31 25

Ex tabulis *Ludovicianis*, a quibus *Carolinæ* hoc in paſſu vix differunt, hoc temporis ſpatio Mercurius heliocentrice in ecliptica progreditur —— ° ′ ″ 2 39 13

Ergo locus nodi his hypotheſibus ♉ 15 16 13

Quodſi mora centri Mercurii ſupponatur 4h. 32′. erit tunc ☊ —— ♉ 15 15 38

Si vero mora centri ☿ ſupponatur 4h. 34′. erit ☊ —— —— ♉ 15 16 47

Quodſi ponamus diſtantiam Mercurii a terra eſſe ad diſtantiam ☿ a ☉ ut 676 ad 313. ſicuti eam celeberr. *Halleius* definit (vid. *Phil. Tranſ.* N°. 386. ſive *The Abridgment*, by Mr. *Reid* and *Gray*, Vol. VI. *p.* 241.) erit inclinatio ☿ in conjunctione —— —— 0 19 47

Ex hoc arcu atque diſtantia ☿ in ecliptica a ☊ conſequitur tandem inclinatio orbitæ, et quidem, in primo caſu, ubi mora centri ſupponitur 4 h. 33′ —— 0 7 5

Quodſi

	h	'	"
Quodsi mora centri ☿ in disco supponitur 4 h. 32'. erit inclinatio orbitæ	0	7	6
Si vero eadem mora esset 4 h. 34'. tunc inclinatio orbitæ — —	0	7	5

IX. *Observations upon so much of Monsieur* le Monnier *the younger's Memoir, lately presented to the* Royal Society, *as relates to the communicating the* Electric Virtue *to* Non-electrics; *by* Wm. Watson, *F. R. S*

Read Jan. 29. 1746-7.

THE World is much obliged to Monf. *le Monnier* for the many Discoveries he has made of the Power of Electricity; though the Reason of my troubling you with this Paper at this time, is my differing with that Gentleman in the Conclusions which he deduces from several of the Experiments contain'd in his Memoir lately presented to the *Royal Academy of Sciences* at *Paris*, his own Extract of which was lately communicated to the *Royal Society*. *

One of the Questions proposed to be examined is, " In what manner the electric Virtue is to be com-" municated to such Bodies as yet have it not, and " which are not capable of acquiring it by bare " Friction only?" Monsieur *le Monnier* observes hereupon, " That no other Manner is known, by " which the electric Virtue may be communicated, " besides the near Approach of a Body actually pos-" sess'd of the same: That the Rule laid down by Monsieur

* *See* These Transactions, N°. 481, *p.* 291.

" Monsieur *du Fay*, *That Bodies never receive*
" *Electricity by Communication, unless they are*
" *supported by Bodies electric in their own Nature,*
" *does not always take place; and that it is liable*
" *to great Exceptions:*" For, first, in the *Leyden*
" Experiment, the Phial filled with Water is strongly
" electrified by Communication, even when carried
" in the Hand, which is not a Body electric by
" Nature."

To this I answer, that Monsieur *du Fay*'s Rule is confirmed by all the Experiments yet made public, and even by that of *Leyden* quoted by our Author, or what is usually called that of Professor *Muschenbroeck*. For, in this Experiment, is not the non-electric Water contained in and supported by the glass Phial, which is electric in its own Nature? Its being carried in the Hand is no more than its being placed on any other non-electric Body, and therefore is no Proof against the general Position. It is well known, that if the Phial is made non-electric by wetting its Outside, so as not to leave some Inches perfectly dry, between its Mouth and that Part which is wetted, the Water and Phial part with the Electricity as fast as they receive it, unless it is stopped by another Electric *per se*. But of this I treated at large, in a Paper I lately did myself the Honour to communicate.

Secondly, our Author mentions, " That all Bo-
" dies, which are electrified by means of a Phial of
" Water fitted to a Wire, and which has already
" received a great deal of Virtue by Communica-
" tion, all Bodies, he says, placed in any curve
" Line, connecting the exterior Wire and that Part
" of

" of the Bottle, which is below the Surface of the " Water, acquire Electricity without being placed " upon Resin, Silk, Glass, or the like: That thus " a violent Concussion may be given to 200 Men " all at once; who holding each other by the " Hand so form the Curve just mention'd, when " the first holds the Bottle, and the last touches the " Wire with the End of his Finger; and this equally, " whether they are all mounted upon Cakes of Re- " sin, or stand upon the Floor: That the Electricity " has in this manner been carried through a Wire " of the Length of 2000 Toises, or near $2\frac{1}{2}$ *English* " *Miles*; Part of which Wire dragged upon wet " Grass, went over Hedges, Palisado's, and over " Land newly ploughed up."

The Experiments in the second Argument do noways invalidate Monsieur *du Fay*'s Rule; for the Success of them depends upon keeping whatever forms the curve Line mention'd by our Author, whether it consists of Men or Wire, in a non-electric State: And if whatever forms this curve Line acquires any Degree of Electricity more than its original Quantity, which it is well known may be done, by being placed upon originally Electrics, the Effect of the Shock is proportionably lessened. Thus if a Man, standing upon Electrics *per se*, applies his Hand to the Phial of Water, suspended by a Wire to the electrified Gun-barrel as usual, this Person will acquire Electricity, which will be sufficiently perceptible in him, by his attracting light Substances held near his Body, or by his firing inflammable ones, when properly presented to him; if, I say, a Person thus electrified, by applying one

of

of his Hands to the Phial, touches the electrified Gun-barrel with a Finger of his other, let the Phial be ever ſo ſtrongly electrified, he feels but a ſlight Stroke; and this Stroke is greater or leſs, in proportion to the Difference of the Accumulation of Electricity in the Body of the Man, and that of the Water in the Phial. Thus we know from Experiment, that though a conſiderable Quantity of the Electricity, in impregnating the Phial of Water therewith, pervades the Glaſs, yet the Loſs thereof this Way is not equal to what comes in by the Wire: Therefore we will, for the ſake of a more eaſy Method of Explanation, ſuppoſe, that the Phial, when electrified in the moſt perfect manner, contains a Quantity of Electricity equal to 10; that the Man's Body, by ſtanding upon Wax, and touching the Phial with one of his Hands during its Electrification, contains a Quantity equal to 7: Upon his touching the Gun barrel with a Finger of his other Hand, he will receive a ſmall Stroke only equal to 3, the Difference of the Electricity of the Water and that of his Body: And if he touches the Gun-barrel again, without removing his Foot from the originally Electric, the Stroke will be ſcarcely perceptible, on account of his Body being nearly of the ſame Degree of Electricity with the Water in the Phial. So that here we ſee that the Violence of the Shock, to be felt by whatever forms the curve Line, depends upon its being, in the moſt perfect manner, free from any Degree of Electricity more than the original Quantity; which is contrary to the Opinion of our Author.

 Thirdly,

Thirdly, Monsieur *Monnier* tells us, "That the "Water of the Bason of the *Thuilleries*, whose "Surface is about an Acre, has been electrified in "the following manner:

"There was stretched round half the Circumfe-"rence of the Bason an iron Chain, which was in-"tirely out of the Water; the two Extremities of "this Chain answer'd to those of one of the Dia-"meters of the Octagon: An Observer, placed at "one of these Extremities, held the Chain with his "left Hand, and dipped his right at the same time "into the Water of the Bason; whilst another Ob-"server, at the opposite Side of the Bason, held the "other End of the Chain in his right Hand, and a "Phial well electrified in his left. He then caused "the Wire of his Phial to touch an iron Rod, "fixed upright in a Piece of Cork that floated "near the Edge of the Bason. At that Instant "both Observers felt a violent Shock in both "their Arms. The same Fact was again confirmed "by Experiments made upon two Basons at the "same time, that it might appear distinctly, that the "electrical *Effluvia* did really pass along the Super-"ficies of the Water."

The Water of the Bason in this Experiment was no more electrified than the Wire which dragged along the Ground, *&c.* was in the former. When I was first informed, without being acquainted how, that an Acre of Water had been electrified, I was amazed, and told the Gentleman who acquainted me therewith, that if my Idea of Electricity was in the least true, such an Effect could not be produced, without electrifying the whole terraqueous Globe from

from a larger Mass of Matter. And indeed, when I heard Monsieur *le Monnier*'s Paper read, I easily saw the Deception: So that, instead of electrifying the whole Quantity of Water contain'd in the Bason, the Electricity passed only through so much of it as formed a Line between the iron Rod fastened in the floating Cork, and the Hand of that Observer which was dipped in the Water.

These Experiments still more and more establish the Account I lately laid before you of the Electricity's always describing the shortest Circuit between the electrified Water and the Gun-barrel; or (which is the same thing) the Wire of the electrified Phial. And this Operation respects neither Fluids or Solids, as such, but only as they are non-electric Matter. Thus this Circuit, in the preceding Experiment between the Phial and the Wire, consisted of the two Observers, the iron Chain, the Line of Water, and the iron Rod in the floating Cork.

Fourthly, Monsieur *le Monnier* mentions, "That "it has been confirmed, by repeated Comparisons, "that a Bar of Iron, placed in the above-mention'd "Curve, does not at all acquire more Electricity "when it is suspended in silken Lines, than when "it is held in the bare Hand: Whence it appears to "him, that, in this Case, the contiguous non-"electric Bodies do neither partake of, nor absorb "in any way, the Electricity which has been com-"municated."

The curve Line before-mention'd, let it consist of whatever Non-electrics it will, unless the Whole thereof be properly supported, the communicated Electricity cannot be accumulated: So that the sus-

 pending

pending one Part thereof in silk Lines cannot be supposed to produce any Effect.

This Gentleman further observes, " That the Phial of " Water fitted to its Wire does not receive the least De- " gree of Electricity, if its Wire, suspended by a silk " Line, is applied to the Globe in Motion, or if that " Phial is placed upon a dry glass Stand." This Monsieur *le Monnier* takes to be directly contrary to Monsieur *du Fay*'s Rule; especially as the Phial cannot be replete with Electricity, unless, while it is exciting, some non-electric Body touches the Phial below the Water.

That the Phial of Water receives no Degree of Electricity in this Case is not strictly true: It receives as much as any other Mass of Matter of the same Bulk would, under the same Circumstances. For we find, that we cannot highly electrify the Water, unless the Electricity from the Globe be directed through the Water and Phial to the Non-electric in Contact; in which Passage a great Quantity thereof is accumulated, by its not pervading the Glass so fast as it is furnished by the Wire; and therefore we find, that when the Water will contain no more, the Surcharge runs off by the Wire: So that this Experiment, no more than those which precede, contradicts Monsieur *du Fay*'s Opinion; the Thinness of the Glass permitting it, not wholly, but partially, to stop the Electricity. This Matter is explained further under Experiment the first.

I differ from this ingenious Author with Reluctance, inasmuch as I greatly honour him, not only for his Discoveries upon the Subject of Electricity, but also for the Pleasure and Improvement I received

in

in my reading his learned and curious Obſervations in Natural Hiſtory, made in the Southern Parts of *France*, where he accompanied Monſieur *Caſſini de Thury* in meaſuring a Degree of the Meridian. Theſe Obſervations are publiſhed with Monſieur *Caſſini*'s Book: But as the reverſe of ſeveral of the Opinions deliver'd in his Memoir is experimentally found to be true, and as the Diſcovery of Truth, and carefully ſeparating it from Deception, ſhould be the only Aim of our Philoſophizing, I take the Liberty of laying before you my Opinion thereon.

X. *Abſtract of a Letter from Mr.* William Arderon, *F. R. S. to Mr.* Henry Baker, *F.R.S. concerning the* perpendicular Aſcent *of* Eels.

Norwich, July 9. 1746.

SIR,

Read Jan. 29. 1746-7.

WHEN I read, ſome Years ago, what Dr. *Plot* in his Hiſtory of *Stafford-ſhire* relates concerning the Paſſage of Eels acroſs Meadows, in the Night-time, from Pond to Pond, I could hardly forbear thinking, that the Gentleman there mention'd muſt by ſome means or other have been deceived; but what I have lately ſeen with my own Eyes gives me great Reaſon to believe his Account to be ſtrictly true.

On the 12th Day of laſt *June*, whilſt I was viewing the Flood-Gates belonging to the Water-works in this City of *Norwich*, I beheld a great Number of

of Eels ſliding up them and the Poſts adjacent, notwithſtanding they all ſtood perpendicular to the Horizon, and 5 or 6 Feet above the Surface of the Pool below the Water-works. They aſcended theſe Poſts and Gates, until they came into the Dam above: And what makes the Matter appear ſtill more ſtrange, they ſlid up with the utmoſt Facility and Readineſs; though many of the Boards and Poſts were quite dry, and as ſmooth as a common Plane had left them.

I obſerved, that at firſt they thruſt their Heads, and about half their Bodies, out of the Water, and held them up againſt the Wood-work for ſome time: I imagine, until they found the glutinous Matter, which is confined about their Bodies, become ſufficiently thick or viſcid, by being expoſed to the Air, to ſuſtain their Weight: Then would they begin to aſcend directly upwards, with as much Eaſe, ſeemingly, as if they had been ſliding along the level Ground; and thus they continued to do, until they they had got into the Dam above.

XI. *A Dissertation on those fossil figured Stones called* Belemnites; *communicated in a Letter from Mr.* Emanuel Mendez da Costa *to* Martin Folkes, *Esq; Pr. R. S.*

SIR,

Read Jan. 29. 1746-7.

THE Origin and Nature of the *Belemnites* having lately been greatly controverted, I have taken the Liberty to address to you the following Thoughts on that Subject; and beg, Sir, if you think them worthy your Regard, you would lay them before the *Royal Society*, as a due Testimony of the great Respect I have for that Learned and Illustrious Body.

The *Belemnites* is a Fossil of different Magnitudes and Colours, ever regular in Shape, which is either cylindric, conic, or thereunto approaching. Numbers of them have, on one Side only, a Chap or Seam running their whole Length; others have it in Part; and in others it is not at all to be observed: It consists of a talcy Matter, with an Intermixture of Spar or Crystal, disposed in *Striæ* from or near its Centre to its Circumference, and is made up of Crusts inclosing each other, the innermost whereof is as regular as the outermost. Sometimes, tho' seldom, in comparison to the Numbers of the *Belemnites*, in the Centre is a Cavity ever conic, whatever the external Shape of the *Belemnites* be. This conic Cavity is at different times empty, or else filled, either with a solid Body of mineral Matter, Crystal, Stone, *Pyrites*, *&c.* or with

with a regular jointed conic Body, called by Lithologiſt the *Alveolus* of the *Belemnites*; which, tho' conſtantly regular and jointed, is nevertheleſs found compoſed of various mineral or metallic Subſtances.

The *Alveolus* above-mention'd, tho' not fully proved ſuch, yet ſeems, by the Aſſent of moſt of the preſent Naturaliſts, to be a Body of marine Origin; a Shell the nigheſt related to the *Nautilus* Kind: It is concamerated, and even in ſome is diſcover'd another great Characteriſtic of the *Nautilus* Kind, I mean the Gut or *Siphunculus*. Therefore, taking this Body for granted to be of marine Origin (for what Reaſons, or of what Kind, is not my preſent intended Subject to prove) it remains to diſcuſs, Whether this Body became accidentally lodged in the *Belemnites*? or, Whether the *Belemnites* itſelf is alſo of marine Origin, and a Part dependent on its *Alveolus*?

Various have been the Opinions of Lithologiſts concerning the Origin of the *Belemnites*; ſome have even aſſerted them of the Vegetable Kingdom; others, that they are Teeth or Horns of Fiſh, Appendages of Shells, Bodies caſt in Shells of the *Tubuli* Kind, or the very Shells themſelves, Spines of *Echini*, or a kind of ſtrait *Nautilus*. The three laſt Opinions are what I ſhall ſtrive to confute, as they ſeem ſomewhat probable, and are now the moſt prevailing; and prove the *Belemnites* to be a natural Foſſil or *Lapis ſui generis*. I deſire no Recourſe to the Subterfuges uſed by others, but hope, Sir, you will agree with me in the Axiom, that all *Belemnites* are of one and the ſame Origin.

That

That the *Belemnites* are not Teeth or Horns of Fish, I shall refer you to the Letter your late learned Member Dr. *John Woodward* wrote on that Subject to Mr. *Bourguet*, of *Switzerland*, wherein he fully proves the Erroneousness of those Opinions. But a further Argument against their being Teeth, which that learned Naturalist has not touch'd upon, is, that no *Belemnites* have that natural Varnish or Polish, which always covers the Teeth of all Animals; whereas the greatest Part of those fossil Bodies, which we know to be such, as the *Bufonitæ*, *Glossopetræ*, *&c.* are found with that same Varnish or Polish. As for their owing their Form to being moulded in Shells, it will appear contradictory to Reason, when we consider, 1°. Their Constitution to be ever as regular as their Figure; and, 2°. That their inner Layer or *Nucleus* is as equally regular as the outer Crust or whole Body; which Particular could never have happen'd, had they been moulded in Shells; as is evident, by the *Turbinitæ*, *Conchitæ*, and other Bodies, which owe their Figures to that Cause. That the *Belemnites* are not Spines of *Echini*, let us first consider, that no Kinds hitherto discover'd have been ever found to have Spines analogous to these Bodies; nor indeed has any marine Shell whatever such a Texture. The immediate Subterfuge for an Answer to this Objection is, that the Kinds of Shells unknown to Mankind are far more in Number than those yet discover'd. I allow it; but think that cannot be an Argument in the present Case, since no one single Species is yet discover'd with such, nor even any *Genus*, which have Spines analogous to the *Belem-*

nites. Nature bears an Analogy through all her Works; and though all the Species of any one *Genus* is not known to any Man, yet that Analogy neverthelefs capacitates us to judge of thofe undifcover'd by thofe we know. Thus we find of the *Echinus* Kind, all the Species now known are ever found near the Shores; confequently, are not fubject to be eternally hidden from us; as is undoubtedly the Cafe of the *Cornua Ammonis*, and *Conchæ anomiæ*. They are no pelagian Shells, as thofe are; Bays and Harbours are the Places where they are fifh'd; their Structure even evinces the Reafons for it. We may therefore with Probability conclude, that all the *Echinus* Kind are of the fame Nature, and have the fame Way of living; that they only inhabit fuch Places, and that none are pelagian Shells; confequently might have been difcover'd.

I am fenfible there are fome Species of foffil *Echini*; as, the moft common *conoid* or *pileated Echini*, the common *Echini galeati*, the *Echini clypeati*, and fome Kinds of the *Echini ovarii*, &c. which tho' we are certain that they have been marine Shells, yet thofe particular Species are not known in the Sea: But then feveral other Species of that fame *Genus* are. The Cafe of this is quite different, fince not one fingle Species of fuch a *Genus* has ever been found.

The exceffive Bignefs and Thicknefs of Numbers of *Belemnites* defcribed by Authors, *viz.* of near two Feet in Length, and above two Inches in Diameter in the thickeft Part, others of three Feet long, and others as thick and long as a Man's Arm; not to enumerate thofe only under a Foot Length, and of

[illegible] proportionable

proportionable Thicknesses, concludes *Echini* of a vast Bigness, to have a Number of such Spines to move.

The Varieties of the *Belemnites,* how can they quadrate to the Spines of one *Genus* of *Echini* only? Solid *Belemnites, Belemnites* with a single Crust, or like a Tube, with a conic Cavity only; that empty, or otherwise filled with a solid Mass, or with a regular jointed Body, as the *Alveolus* —— *Belemnites* of various Magnitudes and Thicknesses, *&c.* can all these Varieties be imagined to belong to one *Genus* of Shells, which we suppose to exist to maintain a favourite System?

The Number of Species of *Echini* discover'd are numerous; and the Spines of all those agree in having a hollow *Axis,* which runs proportionably from their *Basis* to their *Apex,* quite different to the *Belemnites:* And for their Constitution, a foreign Naturalist, a Member of the *Royal Society,* Mr. *Klein* of *Dantzick,* who has professedly wrote on this Subject, could only find of two Kinds, *viz.* those of a porous Constitution, which he observed only to belong to one *Genus*; and those of a solid shattery Substance, like a talcy Spar not striated; which is the most general, and is exactly the same Constitution as all the fossil Spines, or *Lapides Judaici* are.

Further, the *Lapides Judaici* have, at some times, been found adhering to their *Papillæ* or Tubercles, and with Fragments of their Shells; whereas no Naturalist has ever known to be found fossil either the Shells, or the Fragments of such a *Genus* of *Echinus*; not even any Remains proportionable to

 such

such large Spines.——In whatever manner the greater Part of such Shells may have perish'd (which is unlikely, if we consider their Texture and Strength), some must have escaped, when the Spines are found in such excessive Numbers every-where, and always perfect and regular; whereas the fossil Spines, or *Lapides Judaici*, as they are call'd, as likewise the *Echini* or Shells, and all the fossil Bodies of marine Origin, are found broken and shatter'd in all kinds of Manners.

As for their being Shells of the *Tubuli* Kind, my Reasons against it are; Were the *Belemnites* such, they must be all tubular more or less; or otherwise must have suffer'd some Degree of Petrifaction to fill up their Cavities. The Unreasonableness of that Argument is demonstrated by all *Belemnitæ* being of one and the same Texture and Constitution; tho' Numbers are solid, and Numbers are tubular, in different Degrees. Now one Kind of Petrifaction, or any other Change in the Earth, which they might have undergone, could never have given so regular a Texture and Substance, and cause such different Effects as Solidity and Tubularity. And if, on the other hand, we allow it to be inconsistent, as it is, to form the Idea of a Shell of the *Tubulus* Kind, by a solid Body, without that Body having suffer'd some Change in the Earth, while buried in it, we must either deny all solid *Belemnites* to be such *Tubuli*, and run to Subterfuges, by owning them to be natural Fossils; or else allow a great Inconsistency, to uphold a wrong System.

That the *Belemnites* are not a tubular Case, which is Part of, and covers a Shell of the *Nautilus* Kind,

as

as is its *Alveolus.* The Variety of Circumſtances already alleged of the *Belemnites* ſerve to demonſtrate the Improbability alſo of this Opinion, as it has done of the other two. The Numbers of *Belemnites* of all kinds, ſo plentiful every-where, and the Conſideration of how few are furniſh'd with *Alveoli.*

Numbers, I am ſenſible, have conic Cavities; but that thoſe Cavities never did contain *Alveoli,* is evident; that the Sides of the ſaid Cavities are even, and without any circular or other Impreſſions, which a *Belemnites* that has ever contained an *Alveolus* muſt have; that Body being in cloſe Contact to all Parts of the inveſtient *Belemnites,* muſt conſequently impreſs it with its Concamerations; which Impreſſions muſt be therefore found on the Sides of the Cavities of all *Belemnites* which ever contained them.

As for aſſerting, that all the *Alveoli,* which are now found looſe, were originally lodged in *Belemnites,* it cannot be; without inferring alſo, that all *Belemnites* which are now devoid of *Alveoli,* contained ſuch formerly; which, by ſome external or other Agent, have been forced out and looſened from them.

To conſider ſuch an Agent, we muſt alſo conclude its Force to have been exceeding great, to looſen out the *Nucleus* of a Body in cloſe Contact with all its inveſtient Parts; and ſtrengthen'd further to it by Ridges and Grooves, ſuch a Force muſt have compreſs'd, ſhatter'd, and otherwiſe broken and deſtroy'd the *Belemnites* that contain'd them; which is contrary to Obſervation. Further, forcing out the *Alveolus* might perhaps eaſily have happened to the conic *Belemnites;* which hath a Baſis of a larger

Diameter

Diameter than the Middle, where the *Alveolus* is lodged; but we cannot conceive the same by the cylindric, fusiform, and other *Belemnites*, of which the two Ends or Extremes terminate pointed; while the Middle, where the *Alveolus* is lodged, is thick and swell'd.

To force an *Alveolus* out of such shaped *Belemnites*, it is evident, that the narrow Ends of the said *Belemnites* must be quite forced open, broken, and shatter'd, before a broader and more capacious Body could be forced through, especially to such a brittle shattery Fossil as the *Belemnites* is. The evident Facts to the contrary of this are also too common to insist on, since all these *Belemnites* are ever found regular, perfect, and intire.

Further, let us consider the *Alveoli* which are now found in *Belemnites*, they are very seldom if ever found as mere Shells, but ever differently changed or petrified. They are moulded of Stone, *Pyrites*, Crystal, *&c.* Now it can never be argued, that the contained Bodies can ever be so differently changed or petrified in their Covers or Shells, and those Covers or Shells which admitted such different petrifying Particles to undergo no Change or Petrification whatsoever.

Another Proof against this Opinion, is the diverse Forms of *Alveoli* now discover'd by Naturalists, as conic, cylindric, curved, spiral at the *Apex*, &c. whereas all *Belemnites* which have Cavities have none but conic ones.

These cylindric, *&c. Alveoli* are now found in *Pomerellia* in *Poland*, in the Marble of the Island of *Oeland* in the *Baltick Sea* belonging to *Sweden*, and in the Marble of *Sweden*; in *Gothland*, in

Masses

Maſſes of Building-Stone; in *Ingria*, in ſeveral Parts of *Pruſſia*, *&c.* and are commonly of an immenſe Bigneſs, to ſeveral Feet in Length, and proportionably thick, yet not perfect. For ſuch *Alveoli*, which are only *Nuclei*, we muſt ſuppoſe immenſe large *Belemnites*; and ſuch we have never heard of, ſo with Probability we may conclude none ſuch to exiſt.

I do not doubt the Growth of this Error, of the *Belemnites* being a Part of its *Alveolus*, to have been cauſed by too raſh Concluſions, and too little an Inſight into the foſſil Kingdom; which has propagated that Aſſertion of the *Alveoli* being found only in the *Belemnites*; which Experience daily contradicts, ſince we find them looſe, as well as imbedded in many other foſſil Subſtances, as in Marble, Stone, *&c.* as has been above obſerved.

Theſe are the Arguments which I allege, Sir, for the Improbability of the ſaid Opinions. I could advance a Number of other Proofs; but as I have already extended my Letter beyond a due Length, I beg Leave, before I conclude, only to offer ſome few Reaſons for their being a natural Foſſil, or *Lapis ſui generis*.

The very View of a *Belemnites* ſufficiently evinces its mineral Origin, and ſhews it evidently compoſed of two foſſil Subſtances, a Talc, and a Spar, or baſtard Cryſtal; whereof the former is the Baſis, and from which Principle I do not heſitate to attribute its ſtriated Texture. Moſt of the talcy Bodies are of a fibrous Nature, and ſeveral are compoſed of Cruſts incloſing each other, in the ſame Manner as the *Septa* of the *Ludus Helmontii*, ſome of the *Aſbeſtos* kind, the *Hæmatites* Cruſts, *&c.* Of the *Sta-*

lactites

lactites Tribe there are several, which so intirely approach the Texture and Constitution of the *Belemnites*, that were their Shapes a little more regular, the most experienced Lithologist might easily be deceived: And I remember, when abroad, to have seen such, of a prodigious Bigness, which, tho' I was then somewhat conversant in the fossil Study, I could not help taking for *Belemnites*. I do not therefore wonder, that *Petrus Assaltus, in notis ad Metallothecam Mercati*, p. 282, and *Langius*, *Hist. Lap. figurat. Helvetiæ*, p. 133. should judge them a native figured Fossil, formed in the Earth, of the *Stalactites* kind, if that Term for the *Belemnites* might with Propriety be used.

The Cavities of *Stalactites* in some measure illustrate, and are adequate to the Cavities of *Belemnites*; they are placed in as various Positions, and are only different from them by not being exactly conic. As for the regular Figure of the *Belemnites* being excepted against, I believe few Fossilists will argument that, when we see as perfect regular Figures in the fossil Kingdom as in any other Parts of the Creation; as witness the Salts and Crystals of all Kinds; the rhomboid, hexagonal, columnar, and other *Selenites*; the cubic, octangular, dodecaedral, and other *Pyrites*; the quadrangular Pyramids of Tin, the Rhombs of Iron, Cubes of Lead, and infinite other native Fossils, which would take up Time to enumerate, and which are far more perfect Figures than the *Belemnites* are. Chymical and other Trials and Tests (which I hope to have the Honour to lay before you in some future Letter) demonstrate a greater Certainty of its mineral Origin.

As

As for that marine Body the *Alveolus*, I cannot think otherwise than that it is of the *Nautilus* Kind; and which, at the Concretion or Formation of the *Belemnites*, became accidentally lodged in its Cavity, in the same Manner as all other marine Bodies became lodged in the various fossil Substances we now find them in.

I beg, Sir, to remark, that the searching into the Origin of this Body is not merely curious, but of great Use; since, if its Origin is fully asserted, we are then better capacitated to search into the Properties and Uses of this Stone, which I do not doubt are many, and which we could never have attain'd to, without first having examined this principal Part of its History. I am,

SIR,

London, Dec. 27. 1746.

Your most obedient,

humble Servant,

Emanuel Mendez Da Costa.

XII. *A Letter from* Richard Brocklesby *M. D. and F. R. S. to the* Prefident, *concerning the* Indian Poifon, *fent over from M.* de la Condamine, *Member of the* Royal Academy *of* Sciences *at* Paris.

Honoured Sir,

Read Feb. 5. 1746 7.

THE Subjects of Natural Hiftory are often ftrange and uncommon; but the Authors who have treated on them have not failed, on their Parts, to fupport and raife the Wonder, and once conceived Aftonifhment, by afcribing Properties which never exifted in Nature; thus indulging the Humour of finding a Marvellous in all Things, Truths have been greatly obfcured, and Errors propagated without Number.

It is to this Caufe originally (if I miftake not) we are to afcribe the prodigious Multiplicity of Poifons, and that equally numerous Tribe of Antidotes, treated of by the Ancients in their *Materia medica*; and I fhould be very glad to have found modern Authors always juft to Truth, in the Qualities by them afcrib'd to particular Drugs.

Upon hearing lately Part of Mr. *Juan Antonio de Loa*'s Letter to you, Sir, I was fufpicious Mr. *de la Condamine* had taken fome Facts there upon the Authority of others, or elfe had been himfelf a little too much addicted to that general Byas of Mankind, the Love of Prodigy and Wonder.

In

In order to be better satisfy'd, I dissolved, in a certain Quantity of fair Water, as much of the *Indian* Poison as could be suspended, and let it stand to clear 24 Hours; and, having made a superficial Incision with a Lancet into the Nose of a young Cat, a few Drops were sprinkled on the Wound. The Creature at first discover'd no Marks of Injury receiv'd; yet in half an Hour she seem'd, by mewing more than before, to be sensible of some Pain. Thus she remain'd about 20 Minutes; when at length she shiver'd, was sleepy, soon became convuls'd, and, in about half an Hour, her Limbs were flaccid, and her Belly swell'd. These Symptoms continu'd, till she in a short time expir'd.

Some time pass'd, e'er I sat down to inquire what visible Effects had been produced on the Body. I then separated the Head from its Trunk, and carefully examin'd the Brain, and particularly the Origin of the Nerves; but when I had consider'd it thoroughly, I could not discover any preternatural Appearance in any of these Parts. Having spent near half an Hour in this Inquiry, I open'd the *Thorax*, and, with some Surprize, found the Pulsation of the Heart as regular, as if the Animal were in perfect Health. This Appearance cont'nu'd above two Hours after the Cat's Head was off; but afterwards languish'd, and was much weaker.

I then open'd one Ventricle of the Heart, in which the Blood was somewhat coagulated. This may be thought to be partly owing to the *Medicine*; for, soon after it had produc'd Convulsions in the Creature, I had a Mind to see what Blooding would do,

 and

and with that View cut off the Tail; but, contrary to my Expectation, the Arteries that ſupply it with Blood bled very little; and, upon cutting off the Head, the Carotids and both Vertebrals did not pour out above half a common Spoonful.

But as it might be queſtion'd by ſome, from the Continuance of the Heart's Pulſation, whether the Cat might not poſſibly, if let alone, have recover'd, I pour'd a few Drops of the ſame Solution as before into a ſuperficial Wound of a young Dog, weighing 12 Pounds: The Creature, in leſs than an Hour, ſhiver'd, became ſleepy, was very cold, and ſo ſtupid, that he ſuffer'd himſelf to be often burnt by the hot Aſhes beneath the Grate, where he lay for Warmth.

In this comatous Way he continu'd near four Hours, and then ſhook off his *Stupor*, and was much better. I left him all Night, and found him next Morning quite well, and as hungry as ever. Upon this I made an Inciſion at that time into one of the crural Veins, and pour'd a few Drops of the Solution into it: In leſs than 10 Minutes the Dog gave Signs of great Pain, ſoon ſhiver'd, grew cold, was convulſ'd, and in leſs than 20 Minutes died.

Upon opening him nothing uncommon was found, nor was the Blood in this Creature's Heart ſo thick as in the former. The crural Vein did not bleed from a large Orifice, after the Poiſon was infuſed, though it was likely to do it before.

But, as ſome Authors have ſaid, that Birds in particular are inſtantly deprived of Life, if the leaſt Particle

Particle of certain Poiſons are infuſed into the Blood, I had a Mind to try one Experiment, and to this End infuſed a few Drops of our Solution into a cuticular Wound of a ſmall Bird. This occaſion'd hanging of the Feathers, and a *Stupor*, in leſs than 10 Minutes, and kill'd him in ſomewhat more than fifteen.

I gave about two Drachms of Sugar to another Bird of the ſame kind, and ſhortly afterwards pour'd a little of the Solution into its Mouth; but two Drops had ſcarce touch'd his Tongue before the Creature was convulſ'd, and I could with Difficulty lay him down before all Motion was taken away.

I gave theſe two Birds to two Cats; and whether from eating them or not I don't pretend to ſay, the Cats made ſo uncommon a Noiſe the whole Night, that they diſturb'd the Family's Reſt.

From theſe Experiments we find that the ſuppoſed Specific is of no manner of Uſe, even when the Poiſon is only taken at the Mouth; and from them it may appear probable, that our Poiſon is nearly upon the ſame Footing with white Arſenic in the Cure of the Tooth ach.

Thus, Sir, having ſatisfied myſelf, I thought I could do no leſs than give you an Account of the Reſult of my Trials. If they contain'd any thing that could afford you any Pleaſure, ten times the Trouble I have taken would be amply repaid; but as the Subject itſelf is far from the moſt entertaining, and I am conſcious that others may have carried it on to much better Purpoſe, ſo I have nothing to plead

in Excuſe of this Trouble, farther than that I have the Honour to be with great Reſpect,

SIR,

London, Jan. 14. 1746-7.

Your moſt obliged, and humble Servant,

Richard Brocklesby.

XIII. *A Letter from Mr.* Richard Dunthorne, *to the Rev. Mr.* Cha. Maſon, *F. R. S. and* Woodwardian *Profeſſor of* Nat. Hiſt. *at* Cambridge, *concerning the* Moon's *Motion.*

SIR, *Cambridge*, Nov. 4. 1746.

Read Feb 5. 1746-7.

IN the Preface to my lunar Tables, I hinted, that one Uſe of publiſhing thoſe Tables would be, the aſſiſting of Perſons deſirous farther to rectify the lunar Aſtronomy, by enabling them more readily to compare the *Newtonian* Theory with Obſervations.

Since the Publiſhing thoſe Tables, I have ſpent ſome Time myſelf in that Compariſon; and here ſend you the Reſult, that you may communicate it to the *Royal Society*, if you think it deſerves to be made public.

As the Motion of every ſecondary Planet muſt partake of the Errors in the Theory of its primary, I thought proper, before I undertook the Examination of the lunar Numbers, to compare thoſe of the Sun with Obſervations. I compared ſeveral Sets of Mr.

Mr. *Flamſtead's* Obſervations, after the Method he himſelf teaches, in *Prolegom. Hiſt. Cœleſt*, p. 133, *& ſeq.* which, for many Reaſons, I think the beſt Method hitherto uſed; and, with the Concurrence of a Gentleman well skilled in theſe Matters, determined the mean Motion of the Sun at *Greenwich*, the laſt Day of *December* at Noon, *Anno* 1700, *O. S.* ♑ 20° 43′ 40″ of its Apogee, ♋ 7° 30′ 0″, and the greateſt Equation of the Sun's Centre 1° 55′ 40″; which, I am fully perſuaded, are very near the Truth.

The Theory of the Sun being thus ſettled, I proceeded to examine the Elements of the lunar Aſtronomy. I began with Obſervations of lunar Eclipſes about the Equinoxes, when the Apogee of the Moon was in the Sun's Quadratures; becauſe at thoſe Times I could conceive the Moon's Motion affected with no Inequality, but the annual one, called by *Newton* the firſt Equation, and the elliptic one, called *Proſthaphæreſis*: From a Compariſon of ſuch Obſervations I obtained the Moon's mean Longitude, which came out 1′, at leaſt, greater than in the Tables, and very nearly as *Newton* has it in the laſt Edition of his *Principia*.

I went on to examine the Place and Motion of the Apogee, and Theory of the Increaſe and Decreaſe of the Eccentricity, as well as the greateſt and leaſt Eccentricities themſelves (from the beſt Obſervations, and beſt ſituate that I could procure) all which agreed ſo well with the Tables, about the Sun's mean Diſtances, that I dare venture to make no Alteration therein: Indeed I think the 6th Equation does not ſo well account for the Variation of the Motion of the Apogee, and Change of the Eccentricity.

tricity, according to the greater or lesser Distance of the Sun from the Earth; and therefore I set myself to compute what Change this Difference of the Sun's Action upon the lunar Orbit would introduce in the Moon's Place in every Situation of the Sun and lunar Orbit; and found, after many tedious Computations, that the Sun being in Apogee, this Change, where greatest, would amount to about 4′, and to 4′ 16″, when the Sun is in Perigee. In other Distances of the Sun from the Earth, this greatest Change is proportional to the Difference of the Cubes of the mean and present Distances; and in every Situation of the Moon, and of her Orbit, the present is to the greatest Equation nearly as the Sine of the Excess of the Moon's mean Anomaly above twice the annual Argument to *Radius*. It increases the Moon's Longitude, when the Sun is in his $\left\{\begin{array}{l}\textit{Apogeon}\\ \textit{Perigeon}\end{array}\right\}$ Semicircle, and that Excess $\left\{\begin{array}{c}\text{less}\\ \text{greater}\end{array}\right\}$ than 180°; and diminishes it when otherwise *.

In fine, I compared the Theory of the Moon, as to her Longitude, with several Observations, as well in the Octants and Semi-Octants, as in the Syzygies and Quadratures, and found such an Agreement when the above Corrections were made, as seemed rather to be wished than hoped for, considering the many Inequalities wherewith the Sun's Action disturbs the Motion

* If this Equation be increased and diminished in a direct *Ratio* of the Moon's horizontal Parallax, it will become more exact. And I think, if it were always diminished by a fourth or perhaps a third Part, it would agree better with Observations.

The

Motion of the Moon, and the Defects to which the best Observations I have hitherto met withal are liable.

I have compared 100 observed Longitudes of the Moon with the Tables; *viz.* 25 Eclipses of the Moon, all, except the first, taken from *Flamstead's Historia Cœlestis*, the *Philosophical Transactions*, and the *Memoirs* of the *Royal Academy of Sciences*; the two great Eclipses of the Sun in 1706 and 1715; 25 select Places of the Moon from *Flamstead's Historia Cœlestis*, and 48 of those Longitudes of the Moon computed from *Flamstead's* Observations by Dr. *Halley* (as I suppose) printed in the first Edition of the *Historia Cœlestis*. They are as follows:

… of the Moon, and 2 of the Sun, compared with the Tables corrected as above.

Place of Observation.	A.D.	Apparent Time at Greenwich. h ′ ″		☽'s true Place observed s ° ′ ″	M. Anom ☉ s °	M. Anom ☽ s °	Ann. Arg. s °	☽'s Place computed. s ° ′ ″	Diff. from Observat. ′ ″
Dantzick	1652	Septem. 7	6 21 35	11 25 25 51	2 20	6 2	0 3	11 25 26 10	+ 0 19
	1670	Sept. 18.	14 36 48	0 6 13 29	3 1	6 0	0 0½	0 6 16 37	+ 3 8
Greenwich	1678	Octob 19.	8 21 54	1 6 47 9	4 1	7 16½	2 2	1 6 46 38	− 0 31
	1682	Febr. 11.	10 58 52	5 3 47 3	7 25	6 28½	1 14	5 3 46 14	− 0 49
Paris	1684	Dec. 11	10 47 6	3 1 11 20	5 24	1 8	7 16	3 1 13 7	+ 1 47
Dantzick	1685	Nov. 30.	10 35 30	2 19 40 0	5 13	11 26½	5 25	2 19 39 46	− 0 14
Dublin	1686	Nov. 19.	11 13 11	2 8 11 57	5 2	10 12½	4 4½	2 8 11 28	− 0 29
Greenwich	1689	Sept. 18	14 30 37	0 6 32 8	3 1	4 24	10 8	0 6 34 16	+ 2 8
	1690	March 14.	9 56 30	6 4 37 11	8 25	9 18	3 15½	6 4 37 28	+ 0 17
	1696	May 6.	12 2 0	7 26 53 14	10 18	3 1½	8 28	7 26 53 40	+ 0 26
Paris	1699	March 5.	7 13 44	5 25 52 27	8 16	8 27	3 1	5 25 32 14	− 0 13
☉ New England	1703	Decem. 11.	18 29 8	3 0 49 56	5 23	11 24½	5 23	3 0 48 51	− 1 5
Marseilles	1706	May 0.	21 13 57	1 20 46 44	10 12	6 22	7 6	1 20 46 32	− 0 12
Greenwich		Octob. 10.	7 10 0	0 28 0 20	3 22	5 27	11 25	0 27 58 47	− 1 33
	1707	April 5	13 30 0	6 26 18 33	9 17	11 9	5 3	6 26 19 0	+ 0 36
	1708	Sept. 18.	9 10 25	0 6 39 23	2 1	2 12	8 7	0 6 39 19	− 0 4
Paris	1712	Jan. 12.	7 34 0	4 2 57 47	6 24	1 17½	7 25½	4 2 58 37	+ 0 50
☉ London	1713	Nov. 20.	15 27 30	2 9 58 21	5 3	9 21	3 17	2 9 56 56	− 1 25
Paris	1715	April 21.	21 11 5	1 12 0 22	10 3	6 12½	6 22	1 11 59 36	− 0 46
	1717	March 15.	15 7 4	6 6 23 50	8 26	8 22½	2 27	6 6 23 31	− 0 19
	1719	August 18.	8 22 46	11 5 42 8	1 29	10 28	4 20	11 5 42 39	+ 0 31
	1722	June 17.	13 46 10	9 6 47 42	11 29	5 8½	10 25	9 6 47 32	− 0 10
	1724	Octob. 20.	15 40 40	1 9 0 0	4 2	5 25½	11 22	1 8 58 50	− 1 10
	1729	Febr. 2.	8 42 55	4 25 13 39	7 16	3 25	9 14	4 25 14 40	+ 1 1
		July 28	13 0 0	10 16 15 28	1 9	8 3	2 15	10 16 16 26	+ 0 58
	173[illegible]	June 8.	13 47 51	8 28 9 58	11 19	4 27	10 11	8 28 8 45	− 1 13
	173[illegible]	Nov. 20	0 40 25	2 10 3 54	5 3	7 8	1 24	2 10 0 56	− 2 58

25 Places of the Moon, computed by myſelf from *Flamſtead's* Obſervations, compared with the Tables.

A.D	Apparent Time at Greenwich.		h	′	″	☽'s true Place obſerved. s	°	′	″	M An-om ☉ s	°	M. An-om. ☽. s	°	Ann. Argᵗ. s	°	☽'s Place computed. s	°	′	″	Diff. from Obſervat.	′	″
1684	March	13.	8	9	8	2	28	48	40	8	25	2	25	11	19	2	28	50	44	+	2	4
1693	March	6.	7	22	48	3	16	43	12	8	17	3	11½	11	7	3	16	44	0	+	0	48
	Octob.	11.	18	12	34	3	28	34	2	3	24	2	27	5	15	3	28	38	21	+	4	19
1694	Febr.	27.	10	29	16	4	27	27	31	8	10	3	12	9	20	4	27	26	48	—	0	43
	Auguſt	23	11	13	54	11	0	19	11	2	5	8	2	2	21	11	0	21	41	+	2	30
1694	Septem.	15.	5	34	13	8	27	1	34	2	27	6	10	3	11	8	27	1	41	+	0	7
	Septem.	21.	10	50	31	11	22	47	41	3	3	9	3½	3	17	11	22	49	16	+	1	35
	Decem.	13.	6	2	53	0	6	28	43	5	25	8	26½	6	0	0	6	29	58	+	1	15
1695	Febr.	8.	3	55	22	1	5	14	3	7	21	9	7½	7	22	1	5	12	14	—	1	49
	July	9.	5	56	14	7	2	2	42	0	20	3	14	0	2	7	2	3	4	+	0	22
	Septem.	8.	8	30	26	10	3	0	12	2	20	5	21	1	24	10	2	59	50	—	0	22
1696	January	16.	17	29	2	7	5	4	20	6	29	2	29	5	0½	7	5	6	11	+	1	51
	March	4.	9	8	13	4	12	2	24	8	16	11	8	7	3	4	12	0	59	—	1	25
1697	February	18.	6	29	4	2	20	52	29	8	2	8	19	5	10	2	20	51	7	—	1	22
		19.	7	22	57	3	4	18	16	8	3	9	2½	5	11	3	4	17	54	—	0	22
	Septem.	15.	7	54	40	10	1	7	8	2	28	3	21½	11	9	10	1	5	38	—	1	30
1698	Septem.	8.	11	2	35	11	12	15	6	2	21	3	24	9	23	11	12	15	27	+	0	21
	Novem.	27.	3	49	54	10	11	33	39	5	9	2	3½	0	3	10	11	31	13	—	2	26
1699	March	5.	12	8	23	5	28	15	59	8	16	8	29½	3	1	5	28	16	11	+	0	12
1701	Septem.	28.	6	55	23	9	27	13	17	3	10	9	9	6	8	9	27	12	49	—	0	28
1702	Octob.	16.	6	16	49	10	3	25	41	3	28	8	11½	5	13	10	3	26	27	+	0	46
1703	Septem.	13.	11	58	50	11	28	50	30	2	25	8	28	3	3	11	28	53	30	+	3	0
	Octob	6.	6	28	39	9	27	1	44	3	18	7	4	3	23	9	27	0	38	—	1	6
1706	Octob	10.	11	11	10	1	1	11	15	3	22	6	0	11	25	1	1	10	17	—	0	58
1714	Septem.	6.	6	34	7	9	3	44	13	2	18	3	17	0	0	9	3	45	24	+	1	11

 Places of the Moon, computed by Dr. *Halley*, from *Flamstead*'s Observations, compared with the Tables.

A.D.	Apparent Time at Greenwich.					☽'s true Place observed.				M. Anom. ☉.		M. Anom. ☽.		Ann. Arg^t		☽'s Place computed				Diff. from Observat.		
			h	′	″	s	°	′	″	°	″	s	″	s	″	s	°	′	″		′	″
1689	Novem.	16.	11	59	0	2	6	12	43	4	29	6	1	0	0	2	6	14	40	+	1	57
	Decem.	9.	6	1	0	11	28	48	54	5	21⅓	3	21	0	20½	11	28	48	40	—	0	14
		10.	6	46	35	0	12	46	49	5	22⅓	4	4	0	21½	0	12	47	14	+	0	25
		12.	8	26	33	1	12	13	8	5	24⅓	5	0½	0	23	1	12	10	39	—	2	29
		13.	9	24	30	1	27	38	36	5	25⅓	5	14	0	24	1	27	36	16	—	2	20
1690	January	16.	12	42	0	3	15	14	54	5	28½	6	24	0	27	3	15	15	48	+	0	54
		4.	3	3	46	11	11	12	59	6	17	2	24	1	14	11	11	13	8	+	0	9
		6.	4	30	15	0	8	13	31	6	19	3	20½	1	16	0	8	14	36	+	1	5
		10.	7	59	22	2	6	11	20	6	23	5	14½	1	19½	2	6	10	37	—	0	43
		12.	10	8	49	3	7	5	21	6	25	6	121	1	21	3	7	3	59	—	1	22
		13.	11	14	0	3	22	36	28	6	26	6	25½	1	22	3	22	35	19	—	1	9
	February	2.	2	25	39	0	3	57	24	7	15⅓	3	5	2	10	0	3	56	9	—	1	15
		5.	4	51	10	1	16	31	33	7	18⅓	4	26	2	12½	1	16	34	15	+	2	42
		7.	6	48	17	2	15	58	14	7	20⅓	5	24	2	14½	2	15	59	13	+	0	59
		8.	7	51	54	3	0	56	20	7	21⅓	6	8	2	15½	3	0	56	31	+	0	11
		10.	9	56	26	4	0	55	24	7	24	7	6	2	17	4	0	55	1	—	0	23
		11.	10	52	31	4	15	42	16	7	25	7	20	2	18	4	15	41	49	—	0	27
		14.	13	19	31	5	28	12	40	7	28	9	2	2	21	5	28	11	44	—	0	56
		19.	17	3	55	8	2	23	26	8	3	11	12	2	25½	8	2	21	17	—	2	9
		21.	18	45	37	8	27	1	25	8	5	0	10	2	27½	8	26	59	6	—	2	19

1690	Febr.	22.	19	37	40	9	9	21	32	8	6	0	24	2	28	9	9	23	26	+	1	54
	March	7.	5	50	38	2	26	22	50	8	18	6	12	3	9	2	26	25	20	+	2	30
		11.	9	43	23	4	24	31	45	8	22	8	8	3	13	4	24	33	15	+	1	30
		12.	10	33	9	5	8	31	54	8	23	8	22	3	14	5	8	33	11	+	1	17
		13.	11	18	30	5	22	16	37	8	24	9	5½	3	15	5	22	17	26	+	0	49
		14.	12	2	18	6	5	44	39	8	25	9	19½	3	15½	6	5	45	18	+	0	39
		25.	20	54	54	10	24	54	20	9	6½	2	21	3	25½	10	24	53	20	—	1	0
	April	7.	7	48	46	4	20	8	0	9	19	8	5	4	6	4	20	9	18	+	1	18
		8.	8	36	30	5	3	59	40	9	20	8	19	4	7	5	4	0	34	+	0	54
		9.	9	22	0	5	17	34	42	9	21	9	2	4	8	5	17	35	40	+	0	58
		10.	10	5	19	6	0	54	50	9	22	9	16	4	9	6	0	55	35	+	0	45
		11.	10	47	56	6	14	0	8	9	23	9	29	4	10	6	14	2	13	+	2	5
		12.	11	30	56	6	26	55	56	9	24	10	12½	4	11	6	26	56	55	+	0	59
		13.	12	17	31	7	9	41	19	9	25	10	26	4	12	7	9	42	20	+	1	1
		14.	13	4	0	7	22	16	0	9	26	11	9½	4	12½	7	22	17	34	+	1	34
		15.	13	52	44	8	4	45	10	9	27	11	23	4	13½	8	4	44	47	—	0	23
		18.	16	26	47	9	11	43	52	10	0	1	3½	4	16	9	11	44	53	+	1	1
		20.	18	5	47	10	6	30	55	10	2	2	0	4	18	10	6	33	2	+	2	7
		22.	19	37	30	11	2	8	6	10	4	2	27	4	20	11	2	8	45	+	0	39
		24.	21	6	38	11	29	14	33	10	6	3	23½	4	21½	11	29	14	30	—	0	3
	May	14.	13	30	30	8	25	6	53	10	25½	0	7	5	8	8	25	8	48	+	1	55
		22.	19	41	40	0	6	50	26	11	3½	3	23	5	15	0	6	49	43	—	0	43
	June	3.	6	2	13	5	22	31	45	11	15	8	19	5	25	5	22	32	6	+	0	21
	Septem.	1.	6	48	53	9	2	27	42	2	13½	11	15	8	12	9	2	31	20	+	3	38
		8.	12	16	42	0	1	44	51	2	21	2	22	8	18	0	1	44	18	—	0	33
1691	February	23.	3	55	10	1	19	39	56	8	6	3	16	1	18	1	19	40	45	+	0	49
	March	11.	18	27	51	9	7	30	5	8	22½	10	23½	2	2	9	7	30	23	+	0	18
	Septem.	22.	8	51	3	10	19	26	24	3	4	11	16	7	19	10	19	28	32	+	2	8

a, The Time of the Middle of this Eclipse here set down is from the Beginning and End; but *Hevelius* says he could not observe the Beginning exactly. Several intermediate *Phases* compared together shew the Middle to have been about 4′ sooner; to which the Moon's Place computed is $0^s. 6^\circ. 14'. 3''.$ and Diff. $+ 34''$.

b, *b*, *b*, The Moon's Places, observed on *Feb.* 2. *April* 7. and *May* 22. are computed by myself, from the Observations; there being manifestly Errors, either of the Computation or Press, in those printed in the *Hist. Cœlestis*.

Several observed Latitudes of the Moon, which I have compared with the Tables, shew them to be very near the Truth, both in the Motion of the Nodes, and also in the Quantity and Variation of the Inclination. I am,

SIR,

Your humble Servant,

Richard Dunthorne.

XIV.

XIV. *Extract of a Letter from Mr.* Leonard Euler, *Prof. Mathem. and Member of the* Imperial Society *at* Petersburgh, *to the Rev. Mr.* Cha. Wetstein, *Chaplain and Secretary to His Royal Highness the* Prince *of* Wales, *concerning the* Discoveries *of the* Russians *on the* North-East Coast *of* Asia.

Berlin, Dec. 10. 1746.

Read Feb. 5. 1746-7.

AS you are desirous to hear something more particular concerning the *Russian* Expeditions to the North and North-East of *Asia*, I will here give you an Account of all that has come to my Knowlege relating to the same. But as I should, on the one hand, be very glad that these Observations might give any Light concerning the Passage now sought through *Hudson's Bay*, I should, on the other be very sorry, if Mr. *Behring*'s Opinion, who believed that the new Land he had discovered was joined to *California*, should rather lead us to doubt of the Success of that glorious Undertaking. I wish, however, that a happy Experiment may soon inform us certainly of the Truth. In the mean time you will not be sorry to be acquainted with the Reasons upon which Mr. *Behring*'s Suspicions were founded, notwithstanding the Objections you have been pleased to make, and to communicate to me upon that Head.

First, This new Land, which he fell in with at the Distance of 50 *German* Miles from *Kamschatka* towards the East, was followed by him, and coasted for

for a great Way, tho' I cannot ſay how far: From whence alone it will appear, that an Abatement muſt be made in the Diſtance of 30 Degrees, or thereabouts, which you ſuppoſe to be between the laſt known Head-Land of *California* towards the Weſt, and the fartheſt Extremity of this new diſcovered Land towards the Eaſt.

Secondly, Capt. *Behring* having had the Opportunity of obſerving an Eclipſe of the Moon as *Kamſchatka*, concluded from the ſame, that that Place lay much farther off to the Eaſt, than is expreſſed in any Map; and that, to repreſent it truly, it ought to be tranſferred into the other Hemiſphere, as its Longitude is more than 180 Degrees [Eaſt from the Iſle of *Ferro*]. For this Reaſon Captain *Behring*'s new Land will be conſiderably approached to the laſt known Part of *California*, and will not indeed appear to be many Degrees from it.

What we have therefore ſtill to hope is only, that in this unknown Diſtrict there may be found ſome Streight, by which the *Pacific Sea* may freely communicate with *Hudſon's Bay*; but if it ſhall appear that there is no ſuch Paſſage, it muſt then be concluded, that whatever further Progreſs may happen to be made through *Hudſon's Bay*, the Opening at laſt muſt only be into the *Frozen Sea*, from whence there could be no paſſing into the *Pacific Ocean*, but by the Neighbourhood of *Kamſchatka*; and this Way would without doubt be too long, and too dangerous, to be maſter'd in the Courſe of one Summer.

I very much doubt whether the *Ruſſians* will ever publiſh the Particulars of their Diſcoveries, either ſuch as have been made from *Kamſchatka* to-

wards

wards *America*, or ſuch as have been made upon the Northern Coaſts of *Aſia*. And indeed it is but very much in general that I know the Succeſs of this laſt Expedition. What I do was communicated to me by Order of the Court, from the College of Admiralty, for me to make uſe of it in the Geography of *Ruſſia*, which I was at that time charged with.

They paſſed along in ſmall Veſſels, coaſting between *Nova Zemla* and the Continent, at divers times, in the middle of Summer, when thoſe Waters are open. The firſt Expedition was from the River *Oby*; and at the Approach of Winter the Veſſels ſhelter'd themſelves by going up the *Jeniska*; from whence the next Summer they returned to Sea, in order to advance further Eaſtward; which they did to the Mouth of the *Lena*, into which they again retired for the Winter-Seaſon.

The third Expedition was from this River, to the fartheſt North-Eaſt Cape of *Aſia*. But here they loſt ſeveral of their Boats, and a great Part of their Crew, ſo as to be diſabled from proceeding, and from making the whole Tour, ſo as to arrive at *Kamſtſhatka*.

It was however thought, that a further Attempt was then unneceſſary, becauſe Captain *Behring* had already gone round that Cape, ſailing Northward from *Kamſchatka*.

The *Ruſſians* have not attempted the Paſſage round *Nova Zemla*; but as they have paſſed between that Land and the Coaſt of *Aſia*, and as the *Dutch* did formerly diſcover the Northern Coaſts of *Nova Zemla*, we may now be well aſſured, that that Country is really an Iſland.

XV. *Abſtract of a Letter from Mr.* Wm. Arderon, *F. R. S. to Mr.* Henry Baker, *F. R. S. containing ſome Obſervations made on the* Banſticle, *or* Pricklebag, *alias* Prickle-back, *and alſo on* Fiſh *in general.*

Dear SIR,

Read Feb. 5. 1746-7.

MANY of my Leiſure Hours laſt Summer were employed in attending and making Obſervations on ſeveral Kinds of Fiſh; ſome whereof I with great Care have preſerved alive in glaſs Jars for many Months together.

I ſent you ſome time ago a brief Account of what I had obſerved remarkable in the Dace and Ruff, and am now going to lay before you what I have thought worth Notice in that little common Fiſh called the Prickle-Back.

About the Beginning of laſt *April* I took a Banſticle out of our River, full of Spawn, and put it into one of my glaſs Jars, at the Bottom of which I had placed a ſmall Quanitty of Sand, as I always do in every Veſſel wherein my Fiſh are kept; and about the 20th of *May* it buried its Spawn in the ſaid Sand. I was in Hopes this Spawn would have produced a young Brood, but was unluckily diſappointed; which I impute to its being frequently diſturbed by the pouring in of freſh Water.

For ſome Days after I had catched this Banſticle, it refuſed to eat any thing I could offer it, as is common with all Fiſh I have yet kept; but frequently

giving

giving it fresh Water, and coming often to it, it became so familiar as to eat small Worms I now-and-then threw into the Jar, and from that time grew so tame as to take them out of my Hand; nay it became so bold at last, that when its Belly was full, or it did not like what I offer'd, it would set up its Prickles, and with its utmost Strength made a Stroke at my Fingers, if I put them into the Water to it.

This Fish was of so unsociable a Disposition, that it would suffer no other Fish to live in the Jar with it, and so audacious, as to attack whatever I put in, though ten times its own Size.

One Day, for the sake of Diversion, a Friend being then with me, I put a small Ruff into the Jar to it, which the Bansticle immediately assaulted and put to Flight, having in the Conflict torn off a good Part of its Tail; and would, I dare say, have killed it, had I not separated them very soon.

Infinite Numbers of these Prickle-backs are to be found in almost all fresh Waters, where-ever it is possible for Fish to live; and whatever other Kinds the Water is replenished with, this certainly is one, as far as I have yet had Opportunity to make any Enquiry.

The Endeavours they use, and the Ability they have, to get from Place to Place, are also extraordinary; for though the largest of them scarce measures above two Inches in Length, I have seen some of them leap out of the Water a Foot high perpendicularly, and even much further in an oblique Direction, when they wanted to get over Boards or Stones, or some other Obstacle to their Passage,

It is ſcarce to be conceived what Damage theſe little Fiſh do, and how greatly detrimental they are to the Increaſe of all the Fiſh in general amongſt whom they inhabit. For it is with the utmoſt Induſtry, Sagacity, and Greedineſs, that they ſeek out and deſtroy the Spawn of all Sorts of Fiſh; and moreover all the young Fry, that come in their Way, are purſued by them with the utmoſt Eagerneſs, and ſwallowed down without Diſtinction, provided they are not too large.

And in Proof of what I here aſſert, I muſt aſſure you, that the Banſticle before-mention'd in my glaſs Jar, did, on the 4th of *May* laſt, devour, in five Hours Time, 74 young Dace, which were about a Quarter of an Inch long, and the Thickneſs of an Horſe-Hair. Two Days after it ſwallowed 62, and would, I am perſuaded, have eat as many every Day, could I have procured them for it.

Could Gentlemen, who take Pleaſure in Fiſh-ponds, intirely prevent theſe Deſtroyers from getting into them, I am convinced their Produce would be much greater than it commonly is: And though it may not be poſſible to keep them out intirely, it is moſt certainly adviſeable to be very diligent in the deſtroying of them: And whenever, by Netting, or other means, any of them are got out of the Water, never throw them in again, on a Suppoſition of their being harmleſs.

Nature has furniſhed this little Fiſh with a kind of Breaſt-plate or Armour, to be its Defence againſt any outward Injury: She has likewiſe beſtowed upon it ſeveral offenſive Weapons or Spines, placed upon its Sides and Back, which it immediately

erects

erects upon the least Appearance of Danger, or when it attacks some other Fish. The Sharpness of these Prickles guards it well enough from larger Animals, that might otherwise prey upon it; but neither these, nor all the Endeavours it can use, are able to free it from an Enemy that torments it even to Death; what I mean is a kind of Louse, of an oval Figure, having eight Legs, and a very transparent Body, which is able either to swim or crawl, and sticks on it so fast, sucking and plaguing it all the while, that it makes it almost mad.

One remarkable Particular in this Louse is, that its little fibrillous Fins are always in Motion, whether the Creature be swimming about, or fixed upon the Fish.

ALL Fish regulate their Times of Eating and Abstinence by the Temperature of the Air, and the Quarter from whence the Wind blows; and would those Persons who are Lovers of Angling, take the Pains to keep a few small Fish in Glasses, they might at any time easily foretel, from their taking or refusing Food, what Sport is to be expected, and often save themselves many a weary Step taken to no purpose.

I have always observed, amongst the Fish I keep in Jars, that such as have lived a while together contract so great an Affection for each other, that if they are separated they become melancholy and sullen, and are a long time before they forget the Loss.

About *Christmas* last I put two Ruffs into a Jar of Water, where they lived together until *April*; when, at the Desire of a Friend, I gave one of them

away

away. —— After this Separation the Fish that remained with me was so affected, that for three Weeks it would eat nothing I could give it; and therefore, fearing it would pine to Death, I sent it to the Gentleman on whom I had bestowed its Companion; and what is very extraordinary, upon being put together again, it eat immediately, recovered its former Briskness, and both of them are still alive.

I have made abundance of other Observations on Fish, but shall only add at present, that when they remain supine and unactive, they every now-and-then gape and yawn, as most Land-Animals do, when weary of the Situation they are in. I remain,

S I R,

Norwich, July 9. 1746.

Yours, &c.

W. Arderon.

XVI. *A Supposition how the* white Matter *is produced, which floats about in the Air in Autumn; in a Letter from the same to* Mr. Baker.

S I R, *Norwich*, Aug. 28. 1746.

Read Feb. 26. 1746-7.

HAVING lately a large Spider in my Hand, by chance I let it fall, and it hung by its Thread, as they very commonly do. On holding my Hand very still it readily ascended up it again; and thus, by giving it a Shake, and then holding my Hand still, the Spider ascended and descended

ſcended a great many times. I thought, at firſt, it had ſpun a new Thread at every Deſcent, and was deſirous to have meaſured how long an one I could cauſe it thus to ſpin; but, upon a ſtricter Examination, I very plainly perceived, that whenever it aſcended, it wound its Thread with its Feet into a ſort of Coil, and when it deſcended only ravelled it out again

The Manner how they perform this is diverting enough; but as Spiders may be had almoſt in every Place, and the Experiment is ſo eaſily tried, I ſhall forbear deſcribing it; and only add, that as theſe Coils of Thread are exactly like thoſe floating in the Air towards the End of Summer, I think it is not improbable thoſe are made in the ſame Manner, when Spiders have a Mind to direct their Courſe in the ſame Direction their Threads lie.

XVII. *Some Remarks on the precious Stone called the* Turquoiſe; *by* Cromwel Mortimer, *Sec. R. S.* &c.

Read Feb. 26. 1746-7.

THIS Stone has received its modern Name of *Turcheſia*, and *Turquoiſe*, from its being moſt commonly brought from *Turky* into various Parts of *Europe*. *De Boodt* * ſays, the Colour of this Gem is a Variegation of Green, White, and Blue; and that there are two Sorts of it, the oriental, from the *Eaſt Indies* and *Perſia*, and the occidental.

* Gemmar. et Lap. Hiſt.

occidental, from *Spain*, *Germany*, *Bohemia*, *Silesia*, *&c.*; that in *Persia*, where it is found in greatest Plenty, it adheres to black Stones, as if it were an Excrement or a Transudation from them. A Stone of this sort is seldom found to exceed a Walnut in Size; and he mentions one in the Great Duke's *Museum*, on which the Head of *Julius Cæsar* is engraved, as a very extraordinary Sample: He adds, That he never saw one bigger than an Hazel-nut; that some of the oriental ones have the Faculty of preserving their Colour perpetually, which are called Stones of the old Rock; and that others lose their Colour gradually, and are called of the new Rock. He then gives an Instance of a Turquoise which had lost its Colour upon being laid by some time after its Owner's Death, which recover'd its beautiful Colour upon our Author's wearing it upon his Finger in a Ring.

Cæsius, in his Treatise *de Mineralibus*, p. 601. says, This Stone is called *Turcoïs* by *Mylius*, in his *Basilica chemica*; by *Albertus Magnus*, in his Treatise of Minerals; and by *Rueius*, in his Treatise of Gems: but *Turca*, by *Caussinus de Lapillis symbolicis*. *De Boodt*, and Dr. *Woodward*,* with other modern Writers, take it for the *Callaïs* of *Pliny*. *Salmasius*, in his *Plinian. Exercit.* p. 142. says, Many have mistaken the modern *Turquoise* for the *Cyanus*, but that the *Cyanus* was transparent like the Saphire; whereas the *Turquoise* is a sort of Jasper.

Dr. *Woodward*, in his Letter to Sir *Jo. Hoskyns*, † says, That the *Turcoïs*, or *Callaïs* of *Pliny*, is nothing else, but fossil Ivory tinged with Copper. I do

* Method of *Fossils*. Letters, *p.* 17.

† Ibid. p. 16.

do not deny, that some Stones sold for *Turquois*, and possibly all that the Doctor saw were certainly such; but I imagine those which the Authors call of the old Rock, and in which the Colour is permanent, are real mineral Stones: This Sample now before us seems to shew this, from both the Form and Size: Its Shape shews it not to be Part of any animal Bone; but its botryoïd Form is to me a Demonstration that it is the Product of Fire, which had once melted this Substance; and that when it cool'd, its Surface was formed into Bubbles and Blisters, in the same manner as the *Hæmatitis botryoïdes* or Bloodstone, whose Surface consists of Knobs, resembling a Bunch of Grapes.

That the *Elephas* ἐρυκ]ὸς, or *Ebur fossile* of *Theophrastus* *, said to be of various Colours, I do not in the least deny to be tinctured with Copper, and to be what Dr. *Woodword* calls the *Turquois*: Indeed I suspect it to be what *De Boodt* calls of the new Rock; and says is liable to lose its Colour, which it recovers again from the *Effluvia* of the Person who wears it. I therefore, for Distinction sake, think all these Stones of the Ivory Origin should be called *Pseudo-Turchesiæ*, or bastard *Turquois*; and the other Sort, of which this before us is one, the true or real *Turquois*; for, by Examination in the chemical Way, I find it to be a very rich copper Ore; some of it pounded and dissolved in Spirit of Hartshorn gives a deep Blue; in *Aqua fortis* a fine Green; and an iron Wire put into it was in 1 Hour's time incrusted with Copper: Some of it calcined, without any Flux in a Crucible, run

* See *Theophrastus's* Hist. of *Stones*, translated, &c. *John Hill*, *Lond.* 1746. 8°. p. 94.

to a Slag, or half vitrified Subſtance; whereas the ſame Heat, had it been Ivory or Bone, would have reduced it to a white Aſh like Bone-Aſhes; for I expoſed it to ſuch a Fire as vitrified the Tile that cover'd it. Its Hardneſs and Conſiſtence to an Engraver's Tool ſeems to be the ſame as common white Marble: Its Colour is not mended by Heat, but it grows brittle when red hot.

This Specimen, now ſhewn to the *Society*, was about 12 Inches long, 5 Inches broad, and in ſome Places near 2 Inches thick; rough on the under Side, as though broken off from the Rock it had been affixed to; and the upper Side was compoſed of ſmooth poliſhed Knobs, in Form like to the botryoid Iron Ore.

Sir *Hans Sloane*, in his noble *Muſeum*, has ſeveral Specimens of theſe oriental *Turquoiſes*, all botryoid; eſpecially a Maſs from *China*, about three Inches long, two broad, and near an Inch thick: All which ſeem to be Copper Ores: And he has likewiſe Samples of *Turquoiſes* from *Spain*, and the South of *France*; which are all ſmall, and ſeem really to be Pieces of Ivory tinged with Copper.

XVIII. *A Deſcription of a curious* Echinites; *by Mr.* Henry Baker, *F. R. S.*

Read Feb. 26. 1746-7.

MR. *Baker* takes the Liberty of ſhewing the *Society* a very extraordinary *Echinites*, the like to which he has never ſeen in any *Muſeum*, or found deſcribed by any Author. For the *Echinitæ* uſually met with, are made up either of Chalk are Flint, or ſome ſtony, chalky, or

 ſparry

ſparry Matter, formed within the Shell of the *Echinus*, and taking their Figure thence as in a Mould: Which Shell is oftentimes broken off and gone, but remains at other times impregnated with talcy or ſparry Particles: Whereas the Subject now laid before us is compoſed of a tranſparent cryſtalline Subſtance, which has received its general Figure by having been circumſcribed within the Shell of ſome *Echinus*, and ſhews linear Ridges and Diviſions correſpondent to the Lines and Plates found in this kind of *Echinus*.

Was this all, it would be a very uncommon Production, as theſe Bodies have been very rarely known to be formed of Cryſtal *; but it is render'd much more curious and extraordinary, by having exact Rows and Series of little Cells, all of the ſame regular Figure, though leſſening gradually in Size, as they aſcend from the Baſe upwards. (*Vide* Fig. 3.

This Body having been formed within the Shell of an *Echinus*, one would expect (as is the Caſe in all other *Echinitæ* uſually known), that its Figure ſhould be exactly anſwerable to the Mould wherein it was formed; but Mr. *Baker* begs Leave to take notice, that the *Echinus*' Shell is perfectly ſmooth internally, having no riſing Parts correſpondent to theſe Cells or Cavities; and therefore, as it could not receive its Configuration from thence, it muſt be owing to the natural Shooting of the cryſtalline Matter

* Sir *Hans Sloane* has a Maſs, which was form'd within an *Echinus*, the Shell being broken off; it is one Half or Side Cryſtal, the other Side Chalk.

Matter (tho' unlike every thing of that kind yet deſcribed), or to ſome other Cauſe, which he don't pretend to know †.

The Configuration ſeems nevertheleſs in ſome meaſure to correſpond with the Nature of the Shell wherein it was formed: As to the Number of the Rows of Cells, they being ranged by Fives, as the *Papillæ*, *Indentings*, *Lines*, or other *Marks* on the recent Shells of *Echini* conſtantly are; theſe Rows are twenty in Number; *viz.* five double Ranks of large and extremely regular Cells, as at *a a*, &c. between which lie five other double Rows of ſmaller and leſs diſtinct *Cellulæ*, ſhewn at *b b*, &c. Theſe Cells, which are hexagonal, and whereof thoſe in every Row lie alternately to thoſe of the next (by which means they fill up the whole Space), decreaſe in their Size gradually, as they approach nearer to the Top; all the Rows at laſt almoſt concentring at the *Apex*, leaving only a ſmall Space or Vacuity, where in the Shells themſelves of this kind of *Echinus* there is an Aperture. The ſmooth Part at *A* is formed of a pebbly Stone, bearing the ſame Marks as are uſually found in the Impreſſion of theſe *Echinitæ* dug up in Gravel-Pits; which proves, that this muſt have receiv'd its general Figure from one of thoſe Shells, whatever has been the Cauſe of this remarkable Configuration of the cryſtalline Part.

This curious *Echinite* was found in a Marl-Pit at *Baborough*, about three Miles Weſt of the City of *Norwich*, and preſented to Mr. *Baker* by Mr. *Wm. Arderon*, F. R. S.

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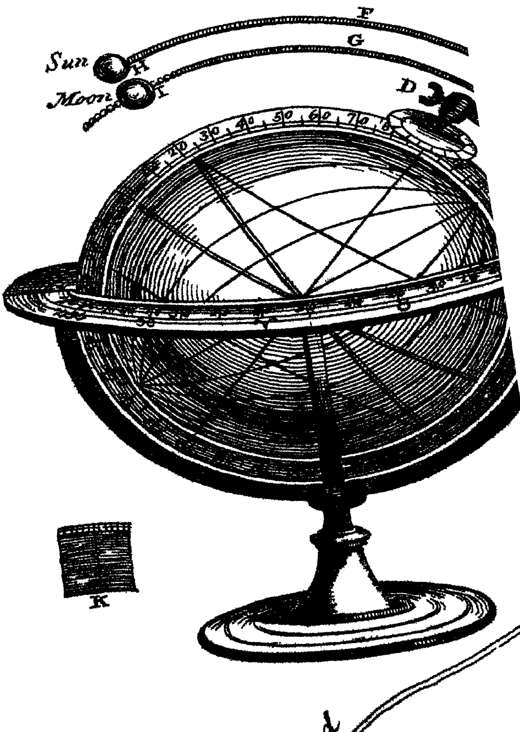
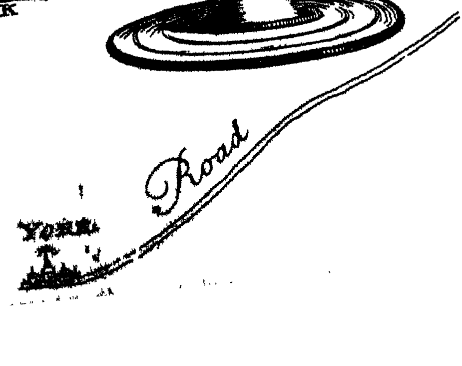
† Perhaps to ſome Cells or Membranes belonging to the Body of the *Echinus*.

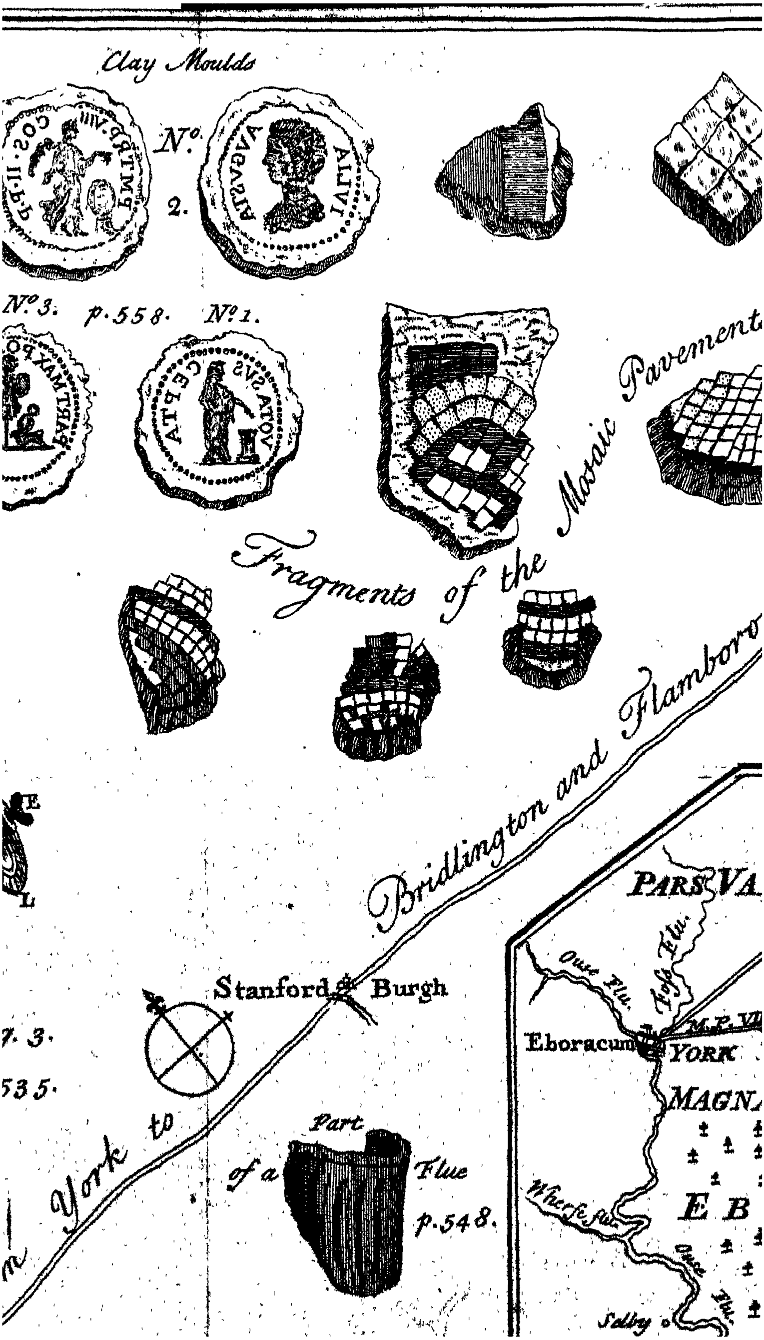
Fig. 1.

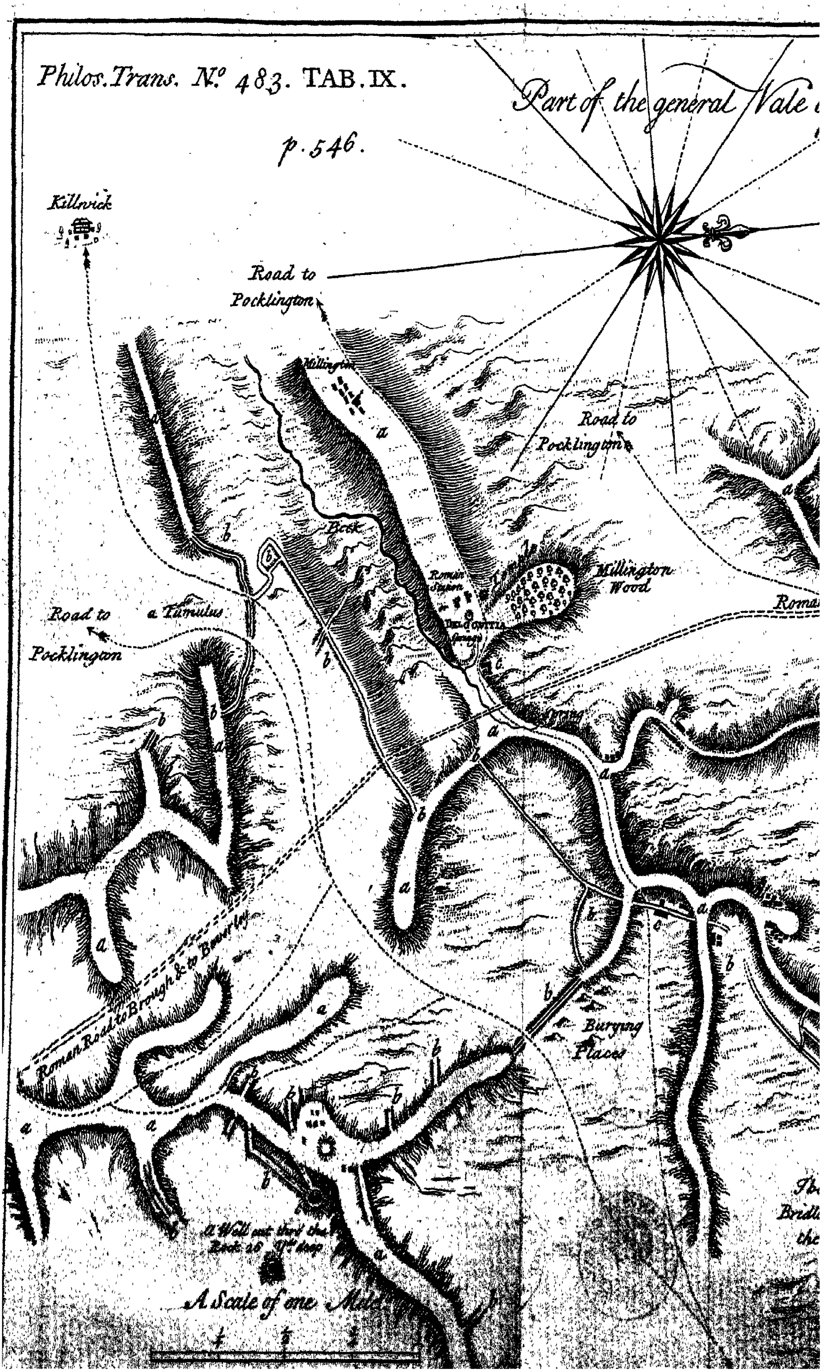
p. 534

Fig. 2.






Clay Moulds
No. 2.
No. 3. p. 558. No. 1.
Fragments of the Mosaic Pavement
Bridlington and Flamboro
York to
Stanford Burgh
Part of a Flue p. 548.
PARS VA
Ouse Flu.
Fosse Flu.
Eboracum
YORK
MAGN
Wherfe flu.
Ouse Flu.
Selby
E B
7. 3.
535.



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p. 546.
Part of the general Vale
Killnwick
Road to Pocklington
Millington
Road to Pocklington
Beck
Millington Wood
Road to Pocklington
a Tumulus
Roman Road to Brough & to Beverley
Burying Places
A Scale of one Mile

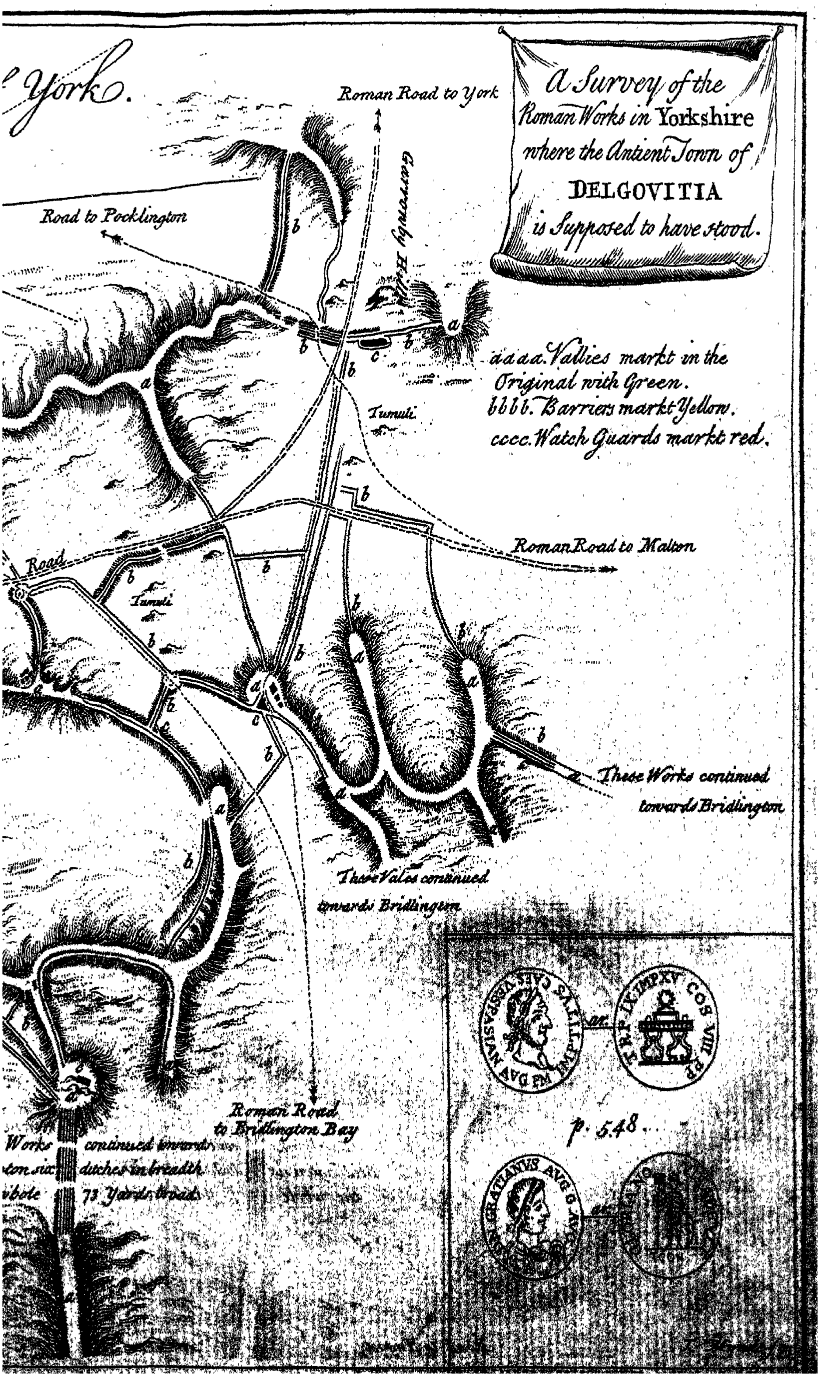

York.
A Survey of the Roman Works in Yorkshire where the Antient Town of DELGOVITIA is Supposed to have stood.
Roman Road to York
Garrowby Hill
Road to Pocklington
aaaa. Vallies markt in the Original with Green.
bbbb. Barriers markt Yellow.
cccc. Watch Guards markt red.
Tumuli
Roman Road to Malton
Road
Tumuli
These Works continued towards Bridlington
These Vales continued towards Bridlington
Roman Road to Bridlington Bay
Works continued towards
ton six ditches in breadth
whole 73 Yards broad
VESPASIAN AVG PM
TR P IX IMP XV COS VIII PP
p. 548
GRATIANVS AVG

Fig 3 · p 519

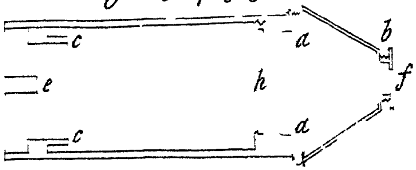


Fig 4 p 519

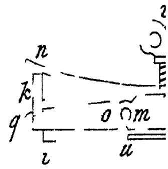
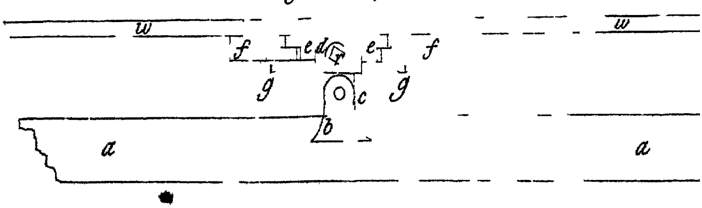


Fig 2 p 518



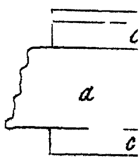


Fig. 1 p 518

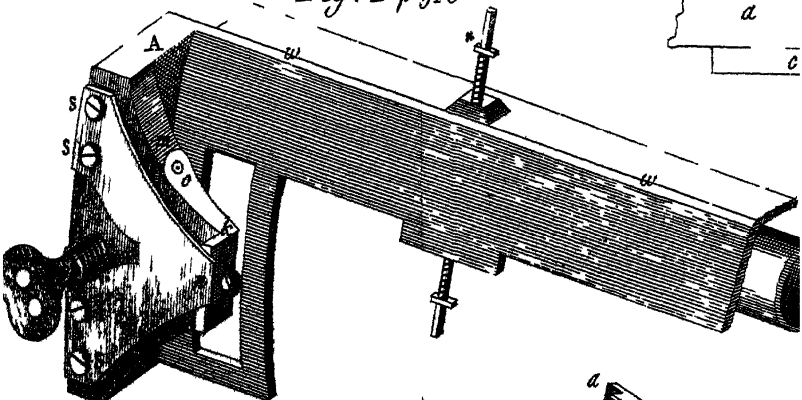


Fig 9 · p 517·

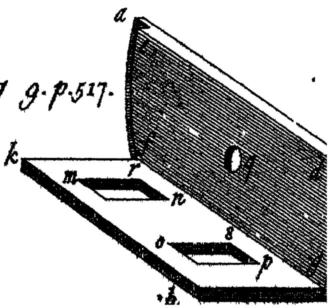


Fig 6 p 516

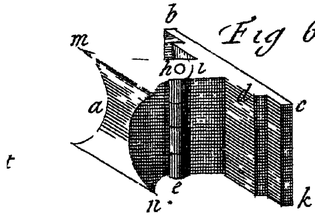


Fig 7 .p 517.

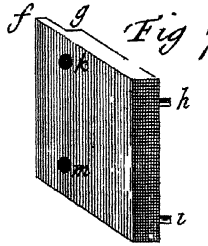


Fig 5 p 517.

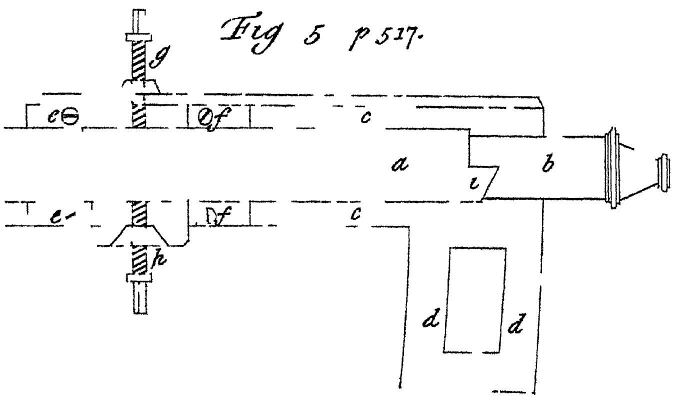
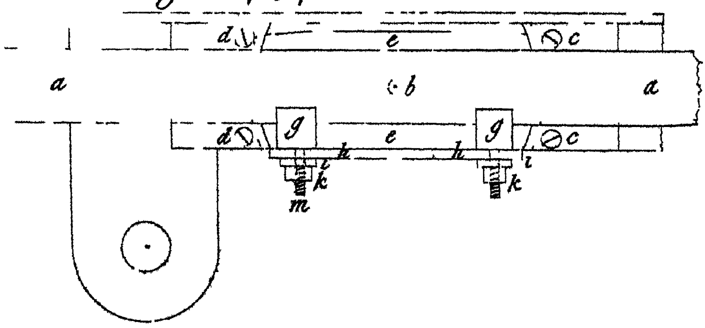


Fig. 8 p. 517.



J. Mynde fc

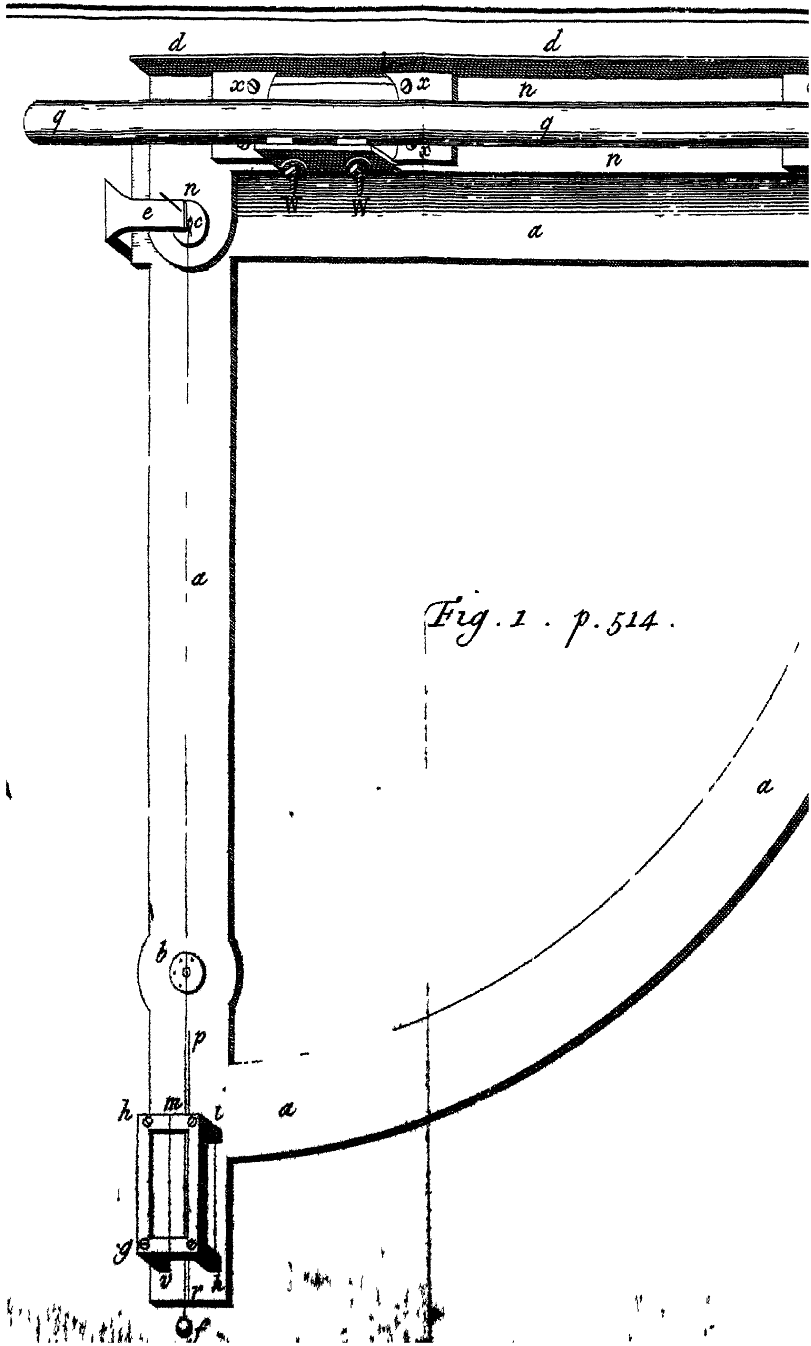


Fig. 1 . p. 514 .

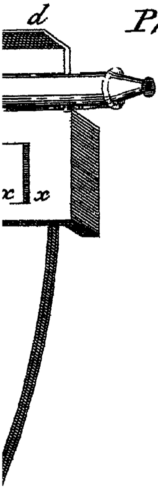


Fig. 2. p. 513

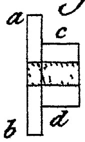
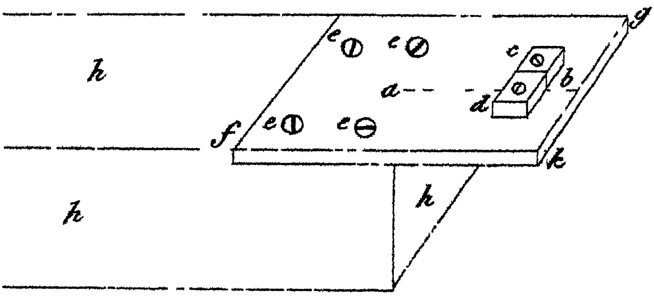


Fig. 3. p. 520.



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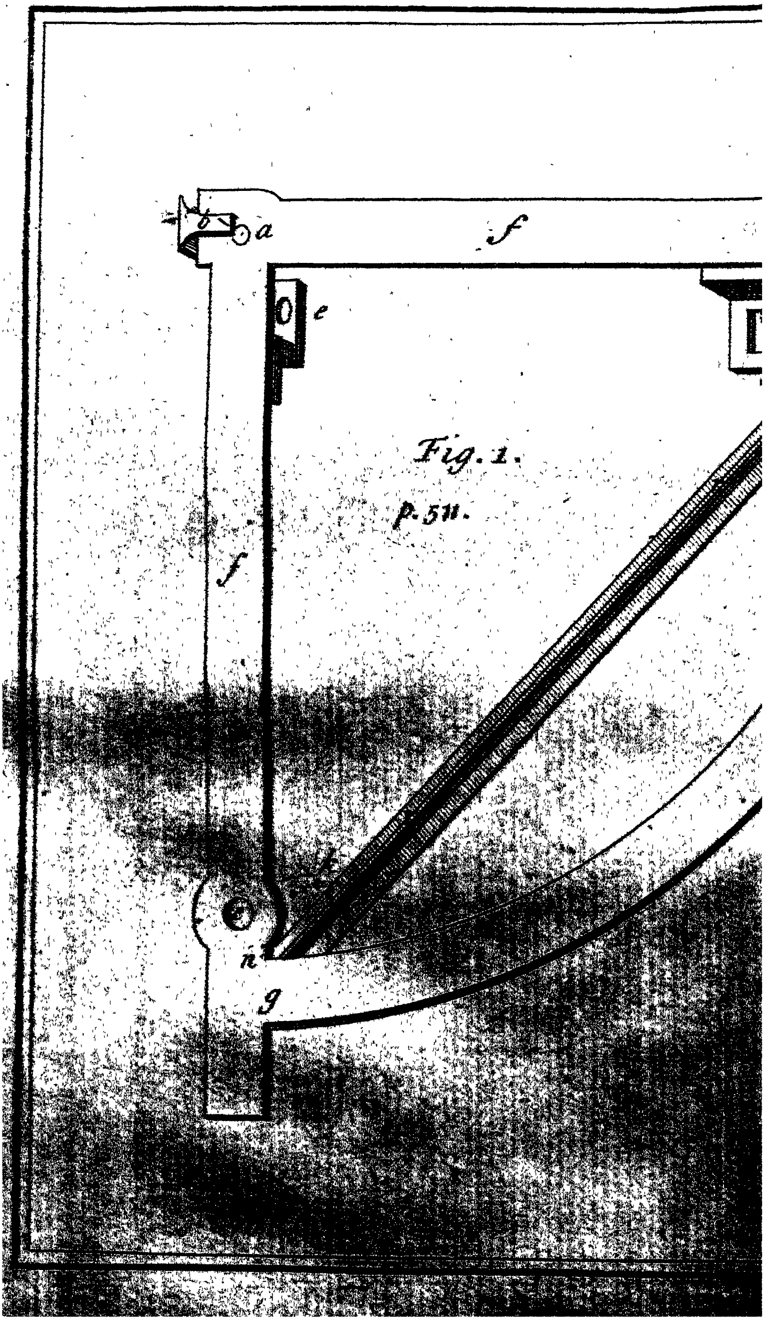
Fig. 1.
p. 511.
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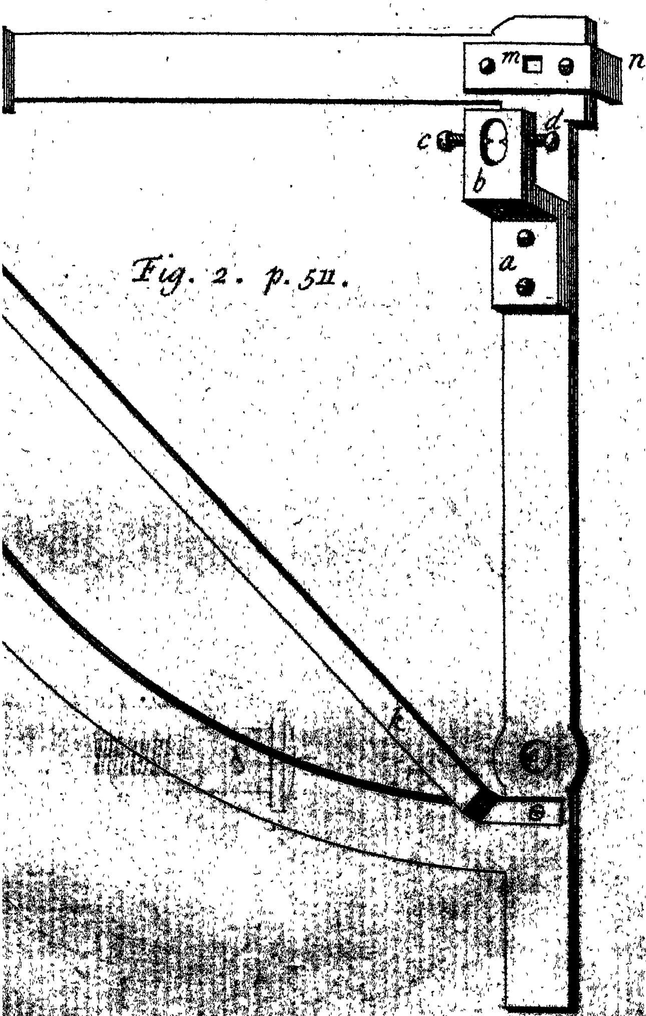


Fig. 2. p. 511.

Fig 1 p 508

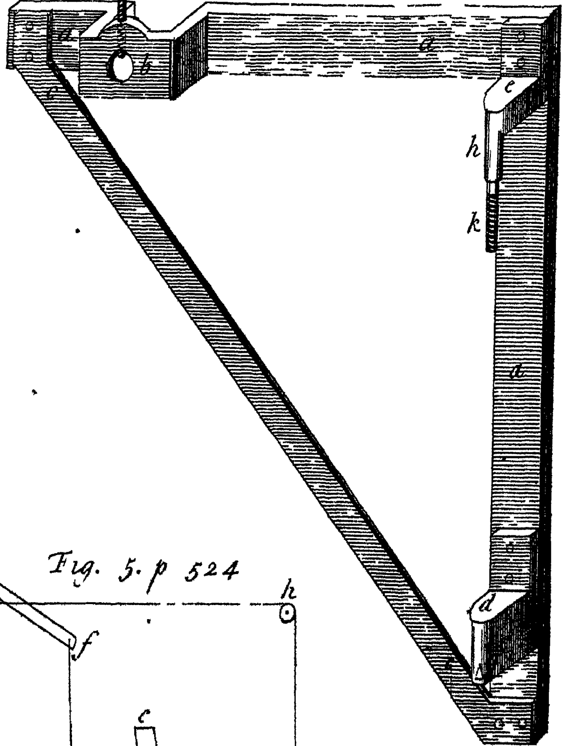


Fig. 5. p 524

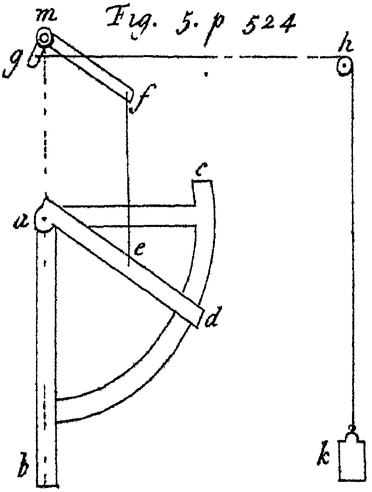


Fig 2 p 508

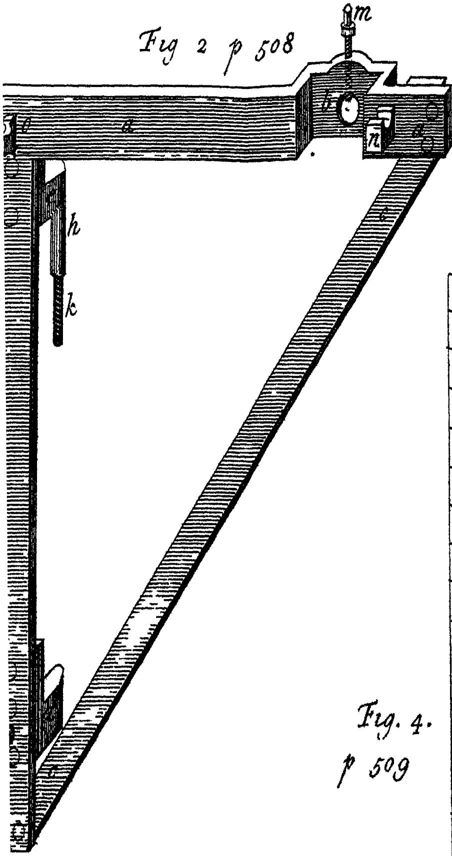
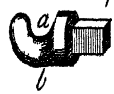


Fig. 3. p. 511.



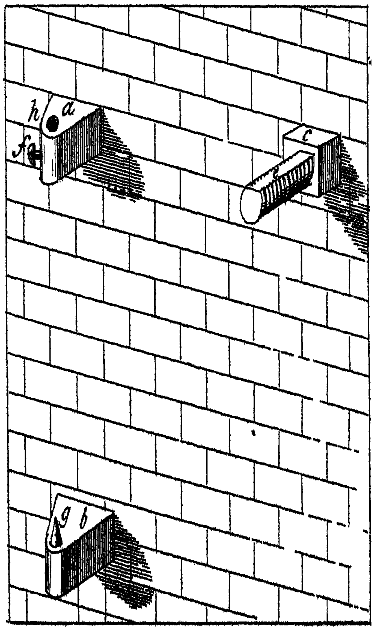


Fig. 4. p 509

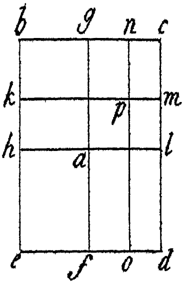


Fig. 6. p. 522.

Fig. 4. p. 499.

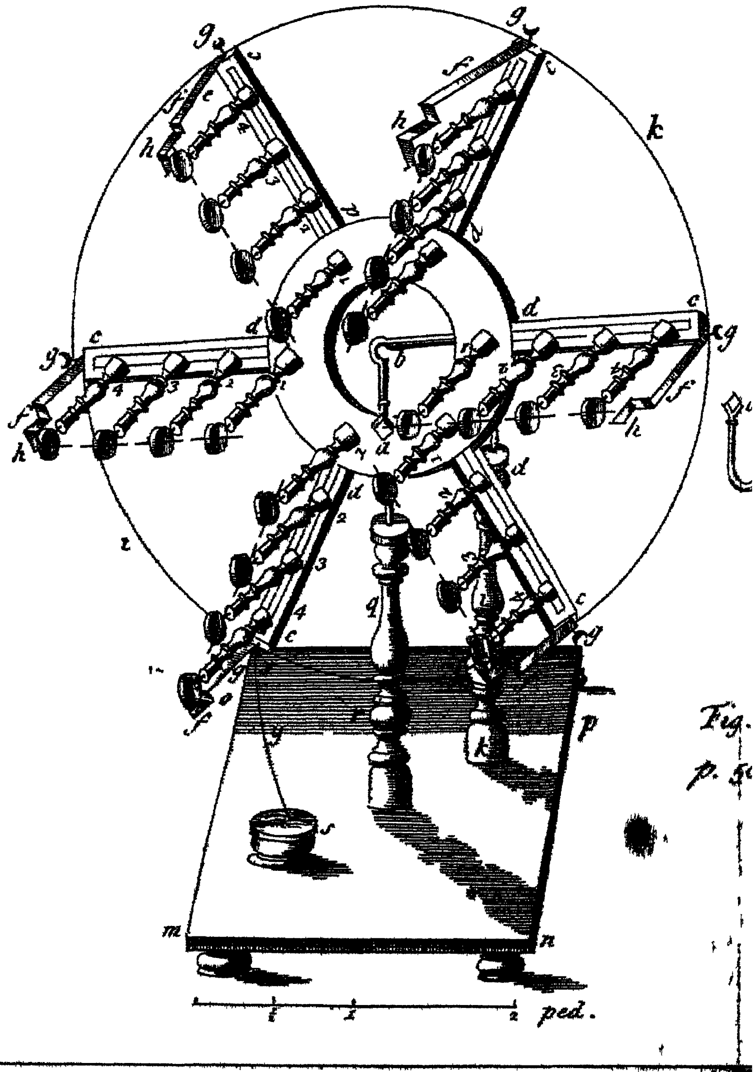


Fig.
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Fig 2 p 498

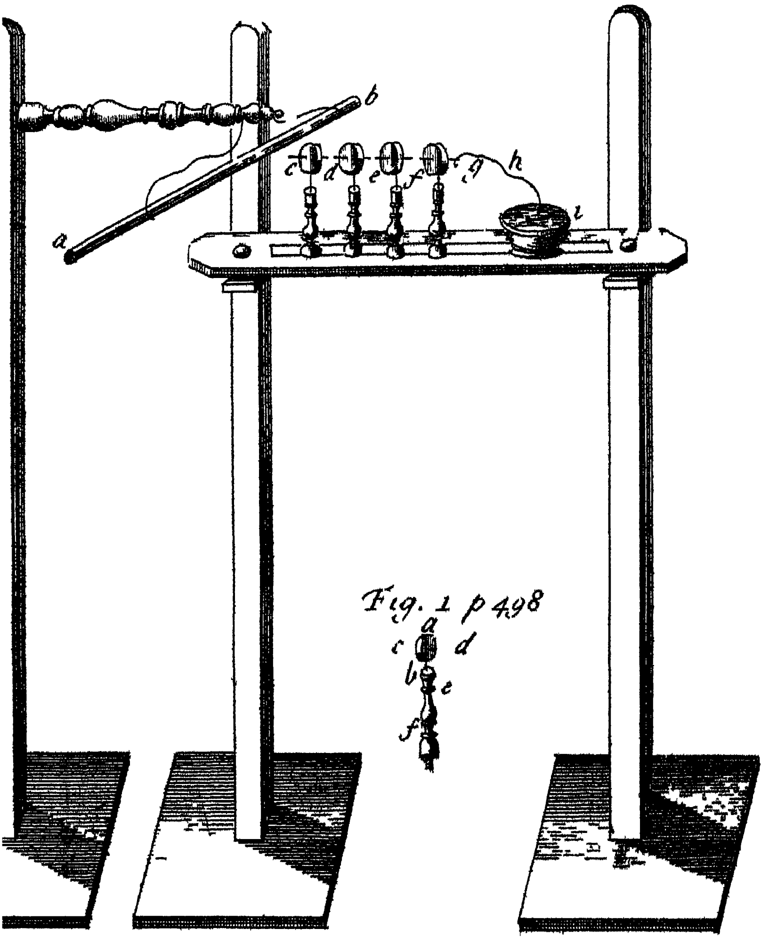

Fig. 1 p 498

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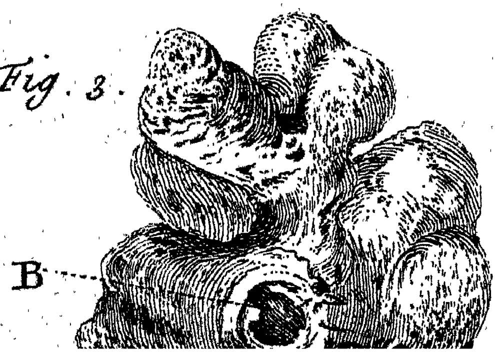
J. Mynde sc.

p. 46.

Fig. 1.

Fig. 3.



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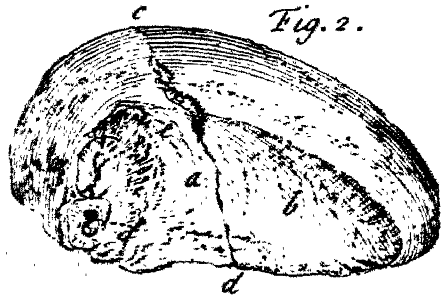
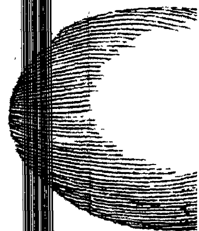




p. 464

Fig. 3.

Fig. [illegible]. p. 451

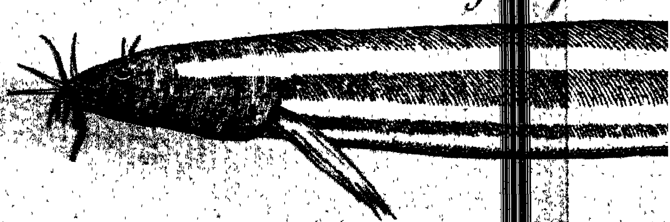
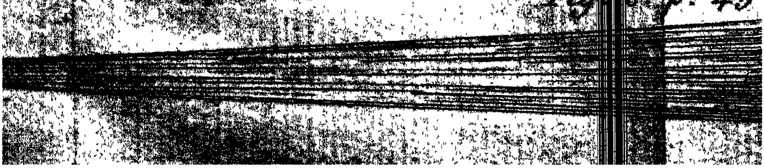


Fig. 8. p. 45



ns. N°. 483. TAB. II.

Fig. 6.

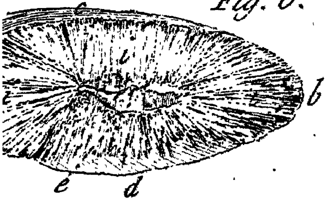
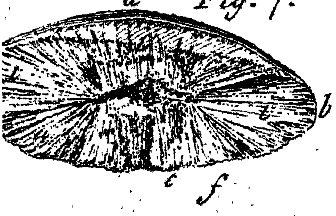
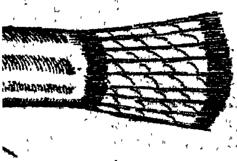


Fig. 7.





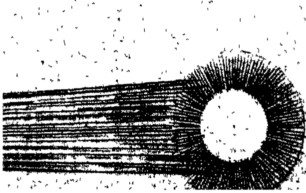
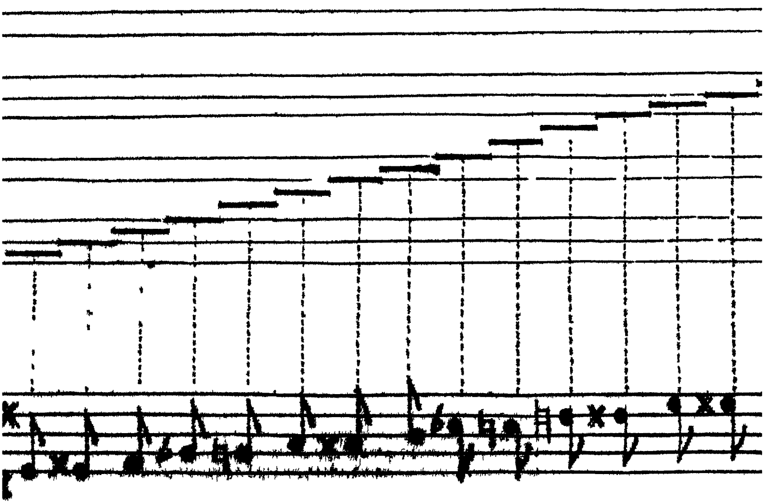


Fig. 1. p. 4.



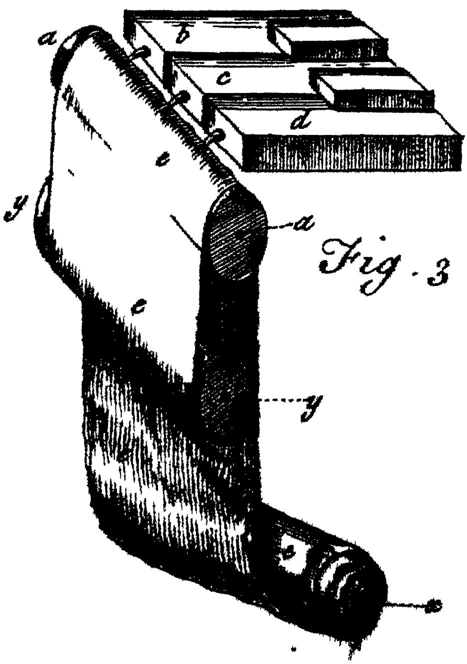


Fig. 3. p. 4.

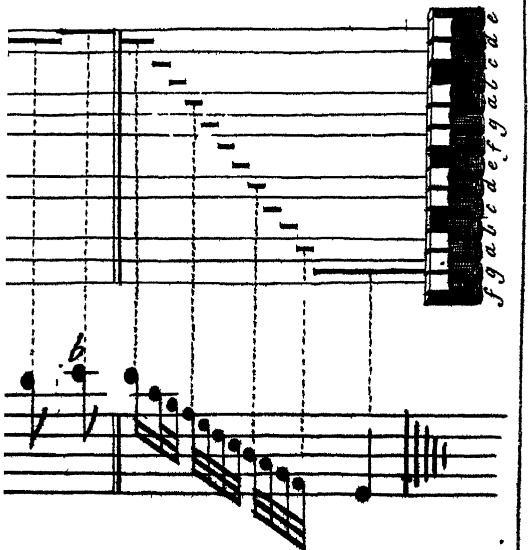
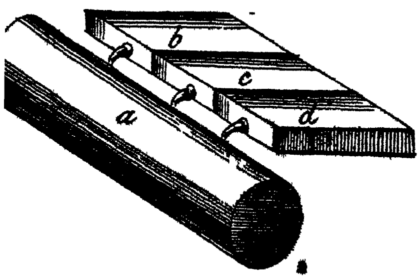


Fig. 2. p. 447.



Numb. 483.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *March*, *April*, and *May*, 1747.

The CONTENTS.

I. *Of* Birds *of* Paſſage, *by Mr.* Mark Catesby, *F. R. S.*

Read at a Meeting of the Royal Society, March 5. 1746-7.

THE Places whereto Birds of Paſſage retreat when they take their Leave of us, are firſt of all to be inquired after; and then it will be proper to examine by what Road, and in what Manner they convey themſelves to ſuch Places whereſoever ſituated on our Globe.

The Reports of their lying torpid in Caverns and hollow Trees, and of their reſting in the ſame State at the Bottom of deep Waters, are ſo ill atteſted, and abſurd in themſelves, that the bare Mention of them is more than they deſerve. Of much the like Stamp is a late broach'd Hypotheſis, which ſends them above our Atmoſphere for a Paſſage to their Retreat; which to me ſeems as remote from Reaſon, as the Ethereal Region is from the Aëreal; through which laſt Region I cannot conceive any Obſtruction to their Paſſage, when, by the Approach of our Winter, they find a Want of Food, and at the ſame time are directed, by Inſtinct, to reſort to ſome other Parts of the Globe, where they may find a freſh Supply. For the Want of Food ſeems to be the chief if not the only Reaſon of their Migration. And tho' Titmice and other ſmall Birds abide here the whole Winter, and ſubſiſt on Inſects, which they find torpid, or in a State of Mutation, in the Crevices of the Barks of Trees, and other their Winter-Receſſes, yet moſt Birds of Paſſage, having tender Bills, are

incapacitated for this Work; but then the Length of their Wings enables them to prey on numberless flying Insects, with which the Air is stored during the warm Months: And it is observable, that not only Swallows, but most other Summer Birds of Passage feed, on the Wing, on such-like Insects as are seen no more when cold Weather begins to come.

The various Conjectures concerning the Places whereto Birds of Passage retire, are occasioned by the Want of ocular Testimony to bring the Matter to some Certainty. But if the Immenseness of the Globe be considered, and the vast Tracts of Land which still remain unknown, unless to their own barbarous Inhabitants, it is no Wonder we are yet unacquainted with the Retreat of these itinerant Birds. If I may be allowed to offer my own Sentiments, I cannot but agree in the general Opinion of their passing to other Countries by the common natural Way of flying, with this additional Conjecture; *viz*, that the Places, to which they retire, lie probably in the same Latitude in the Southern Hemisphere as the Places from whence they depart; where the Seasons reverting, they may enjoy the like agreeable Temperature of Air.

It may be objected, that Places of the same Latitude in the Southern Hemisphere may be divided by too wide a Tract of Sea for them to pass over. But why then may not some other Parts of the Southern Hemisphere serve their Turn? This seems more reasonable to me, than that they should remain on our Side of the Northern Tropic; within a few Degrees of which, at the Winter Solstice, it is so cold, as frequently to produce Snow; which, by dispersing such

ſuch Inſects as Birds, that feed upon the Wing, and particularly the Swallow Kinds ſubſiſt on, muſt make them periſh inevitably, were they not to change their Quarters for thoſe more favourable Climes, where a Continuance of warm Weather affords their natural and proper Food. This their Sagacity dictates to them, and is the apparent Cauſe of their periodical leaving us at the Approach of Winter, before Flies are ſo diſſipated by Cold and Winds as to be found no longer in the Air; tho' they may with other Inſects be met with in Holes and hidden Receſſes, and ſerve to ſubſiſt other Birds of Paſſage.

What I infer from hence is, that as Swallows cannot continue and ſubſiſt ſo long in cold Seaſons as other Birds of Paſſage, they are neceſſitated to viſit us ſomewhat later, and to depart ſooner: For tho' Nightingales, and other Birds of Paſſage, are not often ſeen or obſerved after they ceaſe ſinging, yet I have frequently taken notice of them in their ſolitary Coverts a Month after the Departure of Swallows. From theſe Reaſons I therefore conclude, that Birds of Paſſage, particularly Swallows, are neceſſitated to paſs the Tropic of *Cancer*; but how far more South, or to what Part of the Southern Hemiſphere they go, remains unknown.

The Manner of their journeying to their Southern Abode may vary, as the different Structure of their Bodies enables them to ſupport themſelves in the Air: Thoſe Birds with ſhort Wings, ſuch as the Red-ſtart, Blackcap, *&c.* tho' they are incapable of ſuch long Flights, and with ſo much Celerity, yet I can't ſee why they may not paſs in the like manner, but by gradual and ſlower Movements. Swallows

and Cuckows may probably perform their Flight in half the Time; yet there seems no Necessity for a precipitate Passage, because every Day's Passage affords them Increase of Warmth, and a Continuance of Food a longer Time than is necessary for their Passage, were it to the same Latitude South as that from whence they came.

As Providence in many Instances has guided defenceless Animals to make use of the most necessary Means for their Security, why may not these, and other itinerant Birds, perform their long Journeys in the Night-time, to conceal themselves from rapacious Birds, and other Dangers that Day-light exposes them to; which nocturnal Travelling of Birds of Passage I have Reason to believe more than barely probable, from the following Observation, which may serve in some degree to confirm it:

Lying on the Deck of a Sloop on the North Side of *Cuba*, I, and the Company with me, heard three Nights successively Flights of Rice-Birds (their Notes being plainly distinguishable from others) passing over our Heads northerly, which is their direct Way from *Cuba*, and the Southern Continent of *America*, from whence they go to *Carolina* annually at the time Rice begins to ripen; and, after growing fat with it, return South back again.

The Flight of Birds of Passage over the Seas has by some been consider'd as a Circumstance equally wonderful with other Stories concerning them; and especially in regard to those with short Wings, among which Quails seem, by their Structure, little adapted for long Flights; nor are they ever seen to continue on the Wing for any Length of Time;

and

and yet their Ability for ſuch Flights cannot be doubted, from the Teſtimony of many. *Bellonius* in particular reports, that he ſaw them in great Flights paſſing over and re-paſſing the *Mediterranean* Sea, at the Seaſons and Times they viſit and retire from us.

The ſame Sagacity that inſtructs them to change Climates may alſo reaſonably be thought to direct them, and other Birds of Paſſage, to the narroweſt Part of our Chanel, thereby to evade the Danger of paſſing a wide Sea; tho', by the many Inſtances I have ſeen of Birds driven Hundreds of Miles from any Land, there ſeems not that Neceſſity for their finding the Streights of *Calais*, as the ſhorteſt Paſſage to our Iſland, they being not unable to perform much longer Flights.

There are alſo Winter-Birds of Paſſage, which arrive here in Autumn at the time the Summer Birds depart, and go away in the Spring Seaſon, when Summer Birds return. Theſe however are but few; there being only four Sorts that I know of; *viz.* the Fieldfare, Redwing, Woodcock, and Snipe; which two laſt I have frequently known to continue the Summer here, and breed; ſo that the Fieldfare and Redwing ſeems to be the only Birds of Paſſage that conſtantly and unanimouſly leave us at the Approach of Summer, retiring to more Northern Parts of the Continent, where they breed, and remain the Summer, and at the Return of Winter are driven ſoutherly from thoſe frigid Climates in ſearch of Food, which there the Ice and Snow deprives them of. There are many others, particularly of the Duck and wading Kind, that breed, and make their Sum-

mer

mer Abode in desolate fenny Parts of our Island. When the Severity of our Winters deprives them of their liquid Sustenance, Necessity obliges them to retire towards the Sea in numerous Flights, where in open brackish Waters they find Relief, and at the Approach of the Spring they retire to their Summer Recesses. But these cannot be included among those usually termed Birds of Passage.

Besides the different Kinds of Swallows, I know of but one Kind of *European* Bird that subsists in like manner by catching its Food on the Wing, and that is the *Caprimulgus* or Goat-sucker, the capacious Structure of whose Mouth and Gullet is formed to receive Insects of the larger Kinds, as *Scarabæi*, *Grillo-talpæ*, &c. These are also Birds of Passage.

We have, 'tis hoped, made it pretty evident, that Summer Birds of Passage come to and depart from us at certain Seasons of the Year merely for the sake of a more agreeable Degree of Warmth, and a greater Plenty of Food; both which Advantages they procure by an alternate Change of Climate; but the Migration of Winter Birds of Passage, and particularly of Fieldfares and Redwings, is much more difficult to be accounted for, there being no such apparent Necessity, either on the Score of Food or Climate, for their Departure from us.

The Reason of their coming here in Winter is, 'tis highly probable, for the sake of Food, and a more suitable Climate than that they leave behind them; but, in some severe cold Seasons, and when there is a Scarcity of Berries they subsist here with Difficulty, and are even famished sometimes for Want of sufficient Food; yet what appears most unac-

countable

ountable is, that such as have continued with us a whole Winter in Penury, and should, one would imagine, rejoice at our approaching Spring, and build their Nests, and couple, on the contrary all depart; as if that mild and pleasant Temperature, which delights and cherishes most other Creatures, were disagreeable to them. We know the Places of their Summer Retirement to be *Sweden*, and some other Countries in that Latitude; but, as they would find those Countries too cold for their Reception, and probably destitute of Provision, were they to hasten directly thither when they depart from hence, they journey gradually, and prolong their Passage through the more moderate Countries of *Germany* and *Poland*, by which means they don't arrive at those Northern Regions, adapted by Providence for their Summer Abode, and the Breeding of their Young, till the Severity of the Cold is so much abated as to render it pleasing to them, and Food may be there found. When they visit us again in Winter, their Return back is after the same Manner.

The Winter-Food of these Birds being Berries, and particularly Haws, as a greater Abundance of them grow in this Island than can be supposed in the more Northern Regions, that may possibly be one great Allurement to bring them over hither: But the principal Reason inducing them to travel Southward is probably the Rigour and Severity of the Cold in those frigid Climes, which Nature therefore directs them to desert for such as are more temperate.

A *Swedish* Gentleman informed me some Years ago, that, observing the Use we make of quick-set Hedges

Hedges in *England*, he ſent ſome Plants of the white Thorn over to his own Country for the ſame Purpoſes; before which time he aſſured me there were none of them in *Sweden*, which I mention, in Support of what was ſaid above.

The coming of theſe Birds to us may then pretty well be accounted for from the Reaſons aforegoing; but the Cauſe of their Departure from hence at the time they leave us, is one of thoſe Secrets in Nature which are not yet diſcover'd; for, ſhould it be ſuggeſted, that they do not leave us till the Haws and Berries are all gone, and they are under a Neceſſity to ſeek for Food elſewhere, this would amount to little, unleſs it could be ſhewn, that the Northern Regions to which they journey can afford them a freſh Supply; which it is almoſt certain they cannot. And therefore, when firſt they go from us, they muſt either alter their Diet, or be in much Diſtreſs; but, as 'tis evident their Food in the Summer-time muſt be of a different Kind than what they eat in Winter, 'tis moſt likely they change their Diet; and then one would imagine they ſhould find Subſiſtence here in greater Plenty, and much ſooner than in the colder Countries to which they fly.

In ſhort all, we can be ſaid to know of the Matter, ends in this Obſervation, that Providence has created a great Variety of Birds, and other Animals, with Conſtitutions and Inclinations adapted to their different Degrees of Heat and Cold in the ſeveral Climates of the World (whereby no Country is deſtitute of Inhabitants) and has given them Appetites for the Productions of ſuch Countries, whoſe Temperature is ſuited to their Nature, as well as Know-

ledge

ledge and Abilities to ſeek and find them out: From which we may infer, that the Birds we have been mentioning could no more ſubſiſt in the ſultry Climes of the *Molucca* Iſles, than Birds of Paradiſe could in the frigid Regions of *Sweden* or *Lapland*.

Beſides the migratory Birds already mention'd, which breed and remain the whole Summer, there are other Birds that arrive periodically at certain Places for the ſake of ſome Sort of Grain, or other Food, which may be ſuppoſed their own Country is deſtitute of: Theſe Birds, after no long Continuance, depart, and are no more ſeen till that Time twelve Months after; at which time they return, and ſo continue repeating theſe annual Viſits, as has been already obſerved of the Rice-Bird, and Blue-Wing of *Carolina* (*Nat. Hiſt. Florid. &c.* Vol. I. p. 14, & 99.) Tho' the ſecret Ways by which Inſtinct guides Birds, and other irrational Creatures, are little known to us, yet the Cauſes of ſome of their Actions are apparent.

Analogous to the lucrative Searches of Man thro' diſtant Regions, Birds take diſtant Flights in queſt of Food, or what elſe is agreeable to their Nature; and when they diſcover ſome new Grain, or pleaſing Food, they return and acquaint their Community therewith, and joining in numerous Flights, make annual Excurſions to ſolace on this their exotic Food.

Since the Diſcovery of *America* there have been introduced from *Europe* ſeveral Sorts of Grain, which were never before known in that Part of the World, and which not before ſome Length of Time were found out, and coveted by ſome of theſe migratory

Birds. No Wonder this Grain ſhould not be immediately known to Birds of diſtant Regions; for above half a Century paſſed from the Time of cultivating Wheat, Rice, and Barley, in *Virginia* and *Carolina*, before thoſe Grains were found out and frequented by theſe foreign Birds, of which one has but lately made its firſt Appearance in *Virginia* as my ingenious Friend Dr. *Mitchel* informs me, that he being in his Garden a Bird flew over his Head which appeared with uncommon Luſtre, and ſurpriſed him the more, not having ſeen the like Bird before. Mentioning this to ſome of his Neighbours, he was told by them, what afterwards was confirmed to him by his own Obſervation; *viz.* that theſe exotic Birds had but within theſe few Years appeared in *Virginia*, and had never been obſerved there before.

They arrive annually at the time that Wheat (the Fields of which they moſt frequent) is at a certain Degree of Maturity; and have conſtantly every Year from their firſt Appearance arrived about the ſame time in numerous Flights. They have attain'd the Name of *Wheat-Birds*.

II. *A Letter from Mr.* John Freke *F. R. S. Surgeon to St.* Bartholomew's *Hospital, to the* President *of the* Royal Society, *inclosing a Paper of the late Rev. Mr.* Creed, *concerning a* Machine *to write down* Extempore *Voluntaries, or other Pieces of* Music.

SIR,

Read March 12. 1746-7.

I THINK the inclosed Paper is the Effect of great Ingenuity and much Thought; and as the Subject-Matter of it may tend to give great Improvement and Pleasure to many, not only in our own Country, but every-where, I hope my presenting it may not be thought improper that it may thereby be printed and published to the World.

It was invented and written by Mr. *Creed*, a Clergyman, who was esteemed, by those who knew him, to be a Man well acquainted with all kinds of mathematical Knowlege. It was sent me by a Gentleman of very distinguished Merit and Worth; if therefore from hence this Paper shall be thought proper to be published in the *Philosophical Transactions*. It will prevent its being lost to Mankind. I am,

SIR,

Your very humble

and obliged Servant,

John Freke.

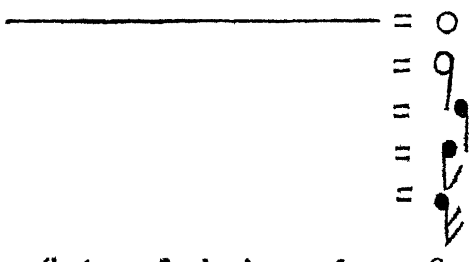
A Demonstration of the Possibility of making a Machine *that shall write* Extempore Voluntaries, *or other Pieces of* Music, *as fast as any Master shall be able to play them upon an* Organ, Harpsichord, &c. *and that in a Character more natural and intelligible, and more expressive of all the Varieties those Instruments are capable of exhibiting, than the Character now in Use.*

Maxim I.

ALL the Varieties those Instruments afford fall under these three Heads: *First*, The various Durations of Sounds, commonly called *Minims*, *Crotchets*, &c. *Secondly*, The various Durations of Silence, commonly called *Rests*. *Thirdly*, The various Degrees of Acuteness or Gravity in musical Sounds, as *A re*, *B mi*, &c.

Maxim II.

Strait Lines, whose Lengths are geometrically proportion'd to the various Durations of musical Sounds, will naturally and intelligibly represent those Durations. *Ex. gr.*

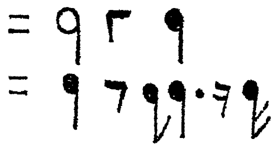


The first Line (being 2 Inches) represents a *Semibreve*.
The second is 1 Inch, and denotes a *Minim*.
The third is half an Inch, and signifies a *Crotchet*.
The fourth is a Quarter, and answers to a *Quaver*.
The fifth is an Eighth, and stands for a *Semiquaver*.

Max m

Maxim III.

The Quantity of the blank Intervals, or Difcontinuity of the Lines, will exactly reprefent the Duration of Silence or Refts. *Ex. gr.*



Maxim IV.

The different Degrees of mufical Sounds, as *Gamut*, *A re*, *B mi*, &c. may be reprefented by the different Situations of thofe black Lines upon the red ones or faint ones. *Ex. gr. fee* TAB. I. *Fig.* 1.

Problem.

To make a Machine to write Mufic in the aforefaid Character as faft as it can be play'd upon the Organ or Harpfichord, to which the Machine is fixed.

Poftulatum.

That a Cylinder may be made by the Application of a circulating, not a vibrating, *Pendulum*, to move equally upon its *Axis* the Quantity of 1 Inch in a Second of Time, which is about the Duration of a *Minim* in *Allegro's*;

Suppofe the Cylinder *a* (fee *Fig.* 2.) to be fuch, and to move under the Keys of an Organ, as *b*, *c*, *d*,

and nail Points under the Heads of the Keys, it is manifeſt, that if an Organiſt play a *Minim* upon *c*, that is, if he preſs down *c* for the Space of a Second, the Nail will make a Scratch upon the Cylinder of 1 Inch in Length, which is my Mark for a *Minim*.

Again, if he reſt a *Crotchet*, that is, if he ceaſe playing for the Space of half a Second, the Cylinder will have moved under the Nails half an Inch without any Scratch; but if the Organiſt next preſſeth down *d* for the Space of half a Second, the Nail under *d* will make a Scratch upon the Cylinder half an Inch long, which is my Mark for a *Crotchet*. It will likewiſe be differently ſituated from the Scratch that was made by *c*, and conſequently diſtinguiſhed from it as much as the Notes now in Uſe are from one another by their different Situation in the Lines. (*Vide Fig.* 1.)

Theſe three Inſtances include all that can be performed upon an Organ, *&c.* (Maxim I.)

Therefore it is already demonſtrated, that whatever is play'd upon the Organ during one Revolution of the Cylinder *a* (*Fig.* 2.) will be inſcribed upon it in intelligible Characters.——I proceed to ſhew how this Operation may be continued for a long time.

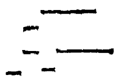
In *Fig.* 3. *aa*, *b*, *c*, *d*, are the ſame as in *Fig.* 2. Let *x* be a long Scroll of Paper wound upon ſuch a Cylinder as *z*. Let *e e e e* be the ſame Scroll brought over the Cylinder *a a*, to be wound upon the Cylinder *y y*, as faſt as the Motion of *aa* (which is determined by a *Pendulum*) will permit.

It is manifeſt, that whatever is play'd upon the Organ during the winding up of *yy* will be written on the Scroll by the Pencils *b*, *c*, *d*, *&c.*

All

All the Graces in Muſic being only a ſwift Succeſſion of Sounds of minute Duration will be expreſſed by the Pencils by ſmall Hatches geometrically proportion'd to thoſe Durations. *Ex. gr.*

A ſingle *Beat*



A double *Beat*

A *Shake*

A *Turn*

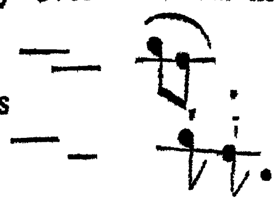
A ſingle *Backfall*

A double *Backfall*

A *Shake* and *Turn*

If a Line commence exactly over or under the Termination of another, it is an Indication of a *Slur*; as

So a ſmall Interval indicates the contrary; as



Flat or ſharp Notes are implied by their Situation on the red Lines; the natural Notes being always drawn between them, *viz.* in the Spaces. (*Vide* Fig. 1.)

The Scroll may be prepared before-hand with red Lines to fall under their reſpective Pencils. It is the ſureſt Way to rule them after; tho' it is feaſible or poſſible to contrive that they may be ruled the ſame Inſtant the Muſic is writing.

The Places of the Bars may be noted by two ſupernumerary Pencils, with a Communication to the Hand or Foot of a Perſon beating Time.

Grave

Grave Music from brisk, flow from fast, *&c.* will be better distinguished by this Machine, than in the ordinary Way by the Words *Adagio*, *Allegro*, *Grave*, *Presto*, &c. for, by these Words, we only know in general this must be flow or fast, but not to what Degree, that being left to the Imagination of the Performer; but here I know exactly how many Notes must be play'd in a Second of Time; *viz.* as many as are contain'd in 1 Inch of the Scroll *per Postulatum* p. 447.

Lastly, Whereas, in the ordinary Way of writing Music, you have either no Character for Graces, or such as do not denote the Time and Manner of their Performance, here you have the minutest Particles of Sound that compose the most transient Graces mathematically delineated.

N. B. Tho', to facilitate the Demonstration, I suppose the Pencils to be fixed under the Heads of the Keys, and consequently to require a very broad Scroll to pass under them; yet I intend the Pencils a more commodious Situation, *viz.* the Motion of the Keys to be communicated by small Rods to them (which I know better how to do than to describe, the Scheme would be so perplex'd). The Pencils are to be made of Steel, and ranged in close Order like the Teeth of a small Comb, so that a very narrow Scroll will do. I can prepare the Paper to receive a very black Impression from the Pencils at so cheap a Rate, that, at the Expence of 6 *d.* in Paper, I can take in Writing all the Music that the swiftest Hand shall be able to play in an Hour.

III. *The Figure of the* Mustela fossilis; *communicated from Dr.* Gronovius *at* Leyden *to Mr.* Peter Collinson, *F. R. S.*

Read March 12. 1746-7. MUSTELA *fossilis, sive Cobites cœrulescens, lineis quinque nigris longitudinalibus.* Arted. *Ichthyol. gen.* xi. 3.*. *Vide* TAB. II. *Fig.* 1.

This Fish was kept alive in a Jar of Water a Year wanting 9 Days, without changing the Water, and without any other Food than what the Water afforded. They dig them out of the Sands near *Wesel* in *Holland.*

IV. *Some Observations on the* Belluga-Stone, *by Mr.* Peter Collinson, *F. R. S.*

Read March 12. 1746-7. THESE Stones of the *Belluga* were collected by Dr. *Cook* at *Astracan*, and sent to Dr. *Sanches* at *Petersburgh*, by whose Favour they came to me. I have applied to those Gentlemen to satisfy my Inquiries about them, and the Accounts they have communicated, with my own Observations, are as follows:

The *Calculus* of the *Belluga* is found of various Shapes and Sizes; it is mostly of a flatted oval Figure,

* Willoughby, *Hist. Pisc.* p. 124. TAB. G. 3, 4. Raii *Syn. Pisc.* p. 69.

gure, sometimes roundish, globular with unequal Depressions, and of a yellowish white Colour externally, and a smooth polished Surface.

It differs in Magnitude, as it does in Figure, from the Bulk of a Pigeon's Egg up to four or five times that Size.

They are mostly compact, ponderous and solid, not very friable, but requiring a pretty smart Blow of a Hammer to break them. They yield easily to the Saw; but this defaces their internal Texture, which is very remarkably elegant and regular. The Stones consist of concentric Coats firmly adhering to each other, formed about a *Nucleus*, which appeared to be quite an heterogeneous Substance, both from its Colour, Hardness, and Texture.

But another obvious Circumstance in its Structure renders the *Belluga* Stone different from most others, which is its radiated Appearance. It seems composed of an infinite Number of shining Rays, regularly diverging from the central *Nucleus* to the Circumference, representing both in Colour and Form the Flakes of a pure white *Terra foliata Tartari*, or (excepting the Colour, which is yellowish) the striated *Spicula* of Antimony.

This Stone is found in the Fish called the *Belluga*, a Species of Sturgeon, the *Acipenser tuberculis carens Artedii*, Part III. pag. 92. It is commonly called *Lapis Bellugæ*, by the *Russians Kamen Belluga*, which signifies the same thing.

Of this Fish several Authors have given us the following Account; in Shape it is not much unlike a Sturgeon,

a Sturgeon (a); only its Snout is proportionably shorter and thicker; the Skin on the Back is light-grey, but under the Belly it is white, and without Scales (b): Its Flesh is whiter than Veal; whence the Name *Belluga*, or the white Fish; and affords a much more delicious Dish (c) than Sturgeon. Of its Row or Spawn is made the *Cavear*; and some are found so large as to yield from 156 to 200 Weight of it. They are found in greatest Plenty, and especially those of the largest Size (d) in the River *Volga*, about the City of *Astracan* (e). *Stratenberg* says, he saw one caught in this River 56 Feet long and 18 Feet thick; and takes them to be the largest River Fish in the World. They are likewise found in other Rivers, as the *Don*, and those that flow into the *Baltick* and *Caspian* Seas.

I am not certainly informed, neither do Authors agree, in what Part of the Fish this Stone is found; *Stralenbergh* says, in the Head and Stomach; some (f) say, in the Air Bladder; others in a particular Bag near the *Anus* or inferior Gut; others again in still different Parts. It is found in both Sexes, but oftenest in the Male, and of all Ages; but is very rare and scarce, for in a thousand Fish it often happens not to meet with a Stone.

From hence it would appear, that these Stones are preternatural to the Fish; perhaps morbid Productions, just as the Stone in the human Bladder, notwithstanding its curious and regular Form; probably

 the

(a) *Vide* Crull's *History of* Russia. (b) Stralenbergh's *History of* Siberia. (c) Crull's *History of* Russia. (d) Stralenbergh *ibid.* (e) Crull's *Hist. of* Russia. (f) *Dr.* Cook's *Letter.*

the Food of the Fish; the Situation of the Parts in which it is generated, and many other Circumstances, may contribute to this Uniformity of Appearance.

A little of this Stone scraped, and laid upon an hot Iron, gave a faint urinous Smell, and calcined into a light, greyish, insipid Earth.

Had it been a real animal Substance, or a constituent Part of the Animal, its Smell would, in all Probability, have at once discover'd it.

The Natives about the *Volga* very much esteem this Stone for its Virtues, being in great Reputation to promote Delivery. The common People take from 10 Grains to 30, 40, or even 60 scraped fine in a little Water, 2, 3, or 4 times in 24 Hours, when the Case is dangerous.

It is also highly commended as a Diuretic and Lithontriptic; and this not only amongst the common People, but amongst such as are more capable of informing themselves of its Effects.

References to the Figures in TAB. II.

Fig. 2. an oval Stone, flat and rugged on its under Side; Part of which *a*, *b*. has been scraped away, and is broken into two Pieces by the Crack *c*, *d*. at *e* and *f* appear 2 *Nuclei* or Centers of smaller Incrustations near the Surface of the larger Stone.

Fig. 3. Is the larger Fragment of the same Stone, or the Side *b*, *c*, *d*. at *g* is a Cavity answering to the Protuberance *h*, in the next *Figure*. *i*, *i*, *i*, are the shining Rays diverging from the central *Nucleus*.

Fig. 4. The smaller Fragment, or the Side *a*, *c*, *d*. in which the smaller *Nuclei* *e* and *f*. of *fig*. 2. appear. *h* is the central *Nucleus*, which fills up the Cavity *g* in *fig*. 3. and *i*, *i*, *i*, are the like Sprays or *Spicula* as in *fig*. 3.

Fig. 5. A smooth oval Stone, in Form of a long Egg.

Fig. 6 and 7. This Stone split in two Pieces, *a*, *b* and *c*, *d*. *e* in *fig*. 6. is the *Nucleus* or Center of the Stone, which seems to have been a Tooth of a Fish, and filled up the Cavity *f* in *fig*. 7. and *i*, *i*, *i*, *i*, are the Sprays or *Spicula* in both Figures.

V.

V. *An Observation on an Occultation of* Cor Leonis *by the Moon, on* Thursday, March 12. 1747, *in* Surrey-Street *in the* Strand, London, *with a reflecting Telescope, made by Mr.* Short, *F. R. S. which magnified about* 100 *times; communicated to the* Royal Society *by* J. Bevis, *M. D.*

Read March 19. 1746-7.

Apparent Time.

	d	h	′	″	
1747, *Mar.*	12	8	24	19	The Star immerg'd into the dark Limb.
-		9	27	4	It emerg'd from the enlighten'd Limb a small Matter to the West of the Moon's Zenith.
			44	4½	The Moon's preceding Limb pass'd the Meridian in the Transitory.
			44	21	The Star passed the Meridian.

Mr. *Short*, another Gentleman, and myself, agreed to a single Second in the Immersion, with different Telescopes; but I saw and pronounced the Emersion 2 or 3 Seconds before them. —— There had been an exact Observation of the Sun's Transit at Noon; and the Clock gain'd about half a Second a Day.

We

We reckon *Surrey-Street* 27 Seconds in Time Weſt of the Royal Obſervatory at *Greenwich*.

Mr. *John Catlin* had a few Days before deliver'd me a Computation of this Eclipſe, corrected from two Places of the Moon, obſerv'd the 28th of *February* and the 2d of *March* 1729, correſponding pretty nearly with her preſent Situation; as likewiſe from the Star's Poſition, as I had rectify'd it from ſeveral late Obſervations; and this gave the

Immerſion at	— —	8	$26\frac{1}{2}$
Emerſion	— —	9	30

J. Bevis.

VI. *An Obſervation of an* uncommon Gleam *of* Light *proceeding from the* Sun, *by Mr.* Peter Collinſon *F. R. S.*

Read March 19. 1746-7.

ON the 8th of *March* 1746-7, near 8 o' Clock in the Morning, as I was riding within three Miles of *Brentwood* in *Eſſex*, there appeared a ſingular *Phænomenon* in the Heavens; the Sketch (Tab. . *Fig.* .) may give ſome Idea of it.

The Morning was fine and clear, the Sun ſhone bright, no Cloud to be ſeen, but the Air a little hazy: Where the *Phænomenon* appeared, which was a bright cloudy Spot, ſeem'd a very ſmall Portion of a Rainbow,

a Rainbow, only the Colours very faint. It was in a horizontal Direction North of the Sun, and from it projected a long luminous Ray, which terminated in a Point.——It continu'd very strong for more than half an Hour after I saw it, and then vanish'd away by degrees.

P. Collinson.

VII. *Extract of a Letter from Mr.* Benjamin Cooke *F.R.S. to Mr.* Peter Collinson *F.R.S. concerning the Property of* new Flanel *sparkling in the dark.*

Newport, Isle of Wight, Jan. 13. 1746-7.

Read March 19. 1746-7.

SINCE I have read the *Transaction* N°. 476. with respect to the sparkling Lady, who could communicate a kind of electrical Fire to her Garments, I can give you an Instance nearly like it, of a Lady who was surprised at such an Appearance from a Flanel Petticoat, which she happen'd to shake in the dark. But at last we found, that new Flanel, after some time wearing, would acquire this Property; but that it lost it by being *washed.*

VIII.

VIII. *A Letter from Mr.* John Hill, *Apothecary, to the* Prefident, *concerning* Windfor Loam.

Read March 19. 1746-7.

AN Accident calling me Yefterday to *Hedgerley*, the Place where there is dug an Earth commonly call'd *Windfor* Loam, and famous not only in *England*, but many other Parts of the World, I took an Opportunity of going to the Pits, and informing myfelf of the prefent Condition of them: And as there appears too much Probability that this Earth will be exhaufted, and loft intirely to the World, in a few Years, I prefume it may not unacceptable to you to have an Account of the Pits of it, and whatever elfe relates to it, taken on the Spot; which I here do myfelf the Honour of communicating to you, and fhall take the Liberty of adding to it what has fince occurr'd to my Thoughts in regard to the fupplying its Place when loft, in the many different Occafions on which it is now us'd.

This Earth itfelf is a coarfe harfh Loam, compofed of a very large fhining Sand, of extreme Hardnefs, and a fine foft tenacious Clay: Its Value is its remarkable Quality of ftanding the Force of the moft violent Fires without running to a Glafs; which makes it extremely ufeful to all who have Occafion for fuch Fires, and is the Reafon of its being fent not only into all Parts of *England*, but to *Holland*, *Germany*, and many other Parts of the World. It is ufed for making the Bricks

Bricks employed in building the Wind-Furnaces for melting Iron, for coating over the Insides of Assay-Furnaces, used by the Workers on Metals, and on many Occasions of like kind at the Glass-Houses, both in *England* and other Nations.

The Place where it is dug is *Hedgerley* before-mention'd, a small Village about 22 Miles from *London*, surrounded with Hills, under one of which this Loam lies. The Pits are about a Quarter of a Mile South-west from the Town, and five Miles North of *Windsor*: They extend over four Acres of Ground, situated on the Descent of a Hill; and were intended to have been carried over much more Ground by the Person who now works them; but, on Trials, the Loam is found not to extend as was imagin'd.

They dig, before they come at this, a very good common Brick-Clay, a Tile-Clay, and a Potter's Earth, a kind of Clay of a firmer Texture, and deeper Colour, than either of those; but the *Strata* of these are seldom pure or regular, and at the Boundaries of the *Stratum* of Loam a pure hard Sand evidently the same with that in the Composition of the Loam, but left loose, from there not having been Clay in the Way to bring it into the Condition of the perfect Mass. They have already work'd the *Stratum* so far as to find it bounded East and West by Beds of this Sand, and Northward by Chalk, and are therefore afraid it will be soon exhausted; at least, whatever they get hereafter must be procur'd with more Labour and Expence, as they have no where to search for it but higher up in the Hill; from whence it must be fetch'd at greater Depths, and much more

Expence; and this increaſing Difficulty of procuring it has been the Reaſon of its riſing in its Price to that it is now ſold at, which is five Shillings a Buſhel in *London*; but which is not to be wonder'd at, ſince on the Spot the Quantity that makes a thouſand Bricks, which us'd to coſt 1*s.* and 8*d.* now coſts 10*s.* the Digging, and will every Year coſt more and more, unleſs a new *Stratum* of it ſhould be diſcover'd ſomewhere thereabouts, which their many unſucceſsful Trials make them at preſent deſpair of.

It is to be obſerv'd, that this valuable Earth forms but a ſingle *Stratum*, and that does not riſe and dip with the Elevation and Deſcent of the Hill, as the *Strata* of the Earth, Stone, *&c.* in Hills uſually do, but ſeems to be even and flat at its Bottom; for the higher up the Hill they open their Pits, the deeper in proportion they find the *Stratum* of Loam lie.

It is worthy Obſervation, that this Hill appears from this not to have been form'd as the Hills and Mountains on the Earth in general have been by a Diſruption and Elevation of the *Strata* by Violence from within the Earth; for, in that Caſe, this *Stratum* of Loam muſt have been elevated with them, and would have been as near the Surface, or nearly ſo, in one Part of the Hill as in another, and need have been dug for no deeper from the Top than from any other Part; whereas, on the contrary, it appears to lie flat and level underneath the whole Maſs of Earth, which makes the Hill, and was, in all Probability, the Surface, on the firſt Settling of the terreſtrial and other Matter from among the Waters of the Deluge.

The

The Earth, which makes the Hill, seems to have been a prodigious Mass of Matter, roll'd along by the irresistible Force of that immense Body of Water, and afterwards lodg'd upon it.

That this might be the Case, the immense Force of that vast Quantity of Water, and the Ease with which heavy Bodies are moved in Water, may serve to make probable; and what the more favours the Conjecture is, that the Earth which makes the Hill is not disposed in such regular pure *Strata* as the Earths settled regularly from the Waters always are, but seems evidently a mixed Mass, made by the jumbling together of various Kinds of Clay, *&c.* which are, in some Parts of it found pure, tho' not in whole *Strata*; and in others irregularly blended in different Proportions one with another; which, as the principal Matters that compose it are of very different Colours, *viz.* a red and a white Clay, is the more apparent. And this is further confirm'd, by there being none of those common extraneous Nodules found lodged in it, which are so frequent in the *Strata* of Clay formed by Subsidence; such as the *Ludus Helmontii*, *Pyritæ*, *&c.* These have settled with, and lodged themselves almost every-where among those *Strata*; but it is no Wonder there are none of them here, if this Hill has been formed, as I imagine; since, in the rolling it along, they must naturally have been left behind: And I promise myself, that the Frequency of these Bodies in almost all our little Clay-pits, and the intire Absence of them in the vast Quantities of Clay that have been dug here, will be esteemed, by all who have look'd deeply into these Studies, one great Argument of

 the

the Truth of this Syſtem; which may alſo extend perhaps to many other Hills as well as this.

As the Workmen are now obliged to dig this Loam at 26 Feet deep, inſtead of about 14, at which Depth they long found it, and muſt hereafter, as they are obliged to aſcend the Hill, dig it at 38 or 40 Feet, the Price of it will, I am afraid, rob us of it, before the Vein is exhauſted. I think it would be a Matter worthy Conſideration, whether, from examining the Parts it is compoſed of, a *Succedaneum* might not be found for it, by an artificial Mixture of ſimilar Subſtances. In order to attempt this, I have, by means of Water, diſunited its Parts, and procured them ſeparate; and, on comparing them with the various Earths and Sands from different Parts of *England*, which I have at times procur'd, I think that I can exactly match the Sand with one from *Hampſtead-Heath*, and the Clay with one from a Pit near the lower End of *Highgate*: The Proportions may be eaſily learned, by accurate Obſervation of the Quantities of each, where diſunited; and a *Succedaneum* on theſe Principles eaſily made. I intend to attempt it, and recommend the Trial of it to Dr. *Mortimer*, who is well acquainted with the genuine, in *Chemical* and *Metallurgic* Operations, in both of which he is very converſant; and I hope to find it equally ſerviceable.

It is evident to me, that the only Reaſon why it endures the Fire ſo much better than other Clays, is the extreme Hardneſs and great Quantity of the Sand it contains: And as I imagine it eaſy to throw a Sand of equal Hardneſs, and in equal Quantity, into

an

an artificial Loam. I see no Reason to doubt of making it equally useful. I am,

SIR,

With all Respect,

Your very obedient

humble Servant,

Broadway Westminster,
May 28, 1746.

John Hill.

IX. *A Letter from Mr.* Rob. Lucas, *concerning the* Relief *he found in the* Stone *from the Use of* Alicant Soap *and* Lime-Water, *to his Brother the Rev. Mr.* Richard Lucas *F.R.S.*

Dear Brother,

Read March 26. 1747.

I HAVE now the Satisfaction to acquaint you, that, by God's Blessing upon the Means His Providence directed me to the Use of, I am so far recovered of my Distemper (no other than the Stone), that I have not the least Doubt of being quite free from it in a little time. I am continually voiding Stones all broken, white on the Outside, without much Pain. I can now walk twice as fast as I could three Weeks since, without Uneasiness; nay I rode the other Day at once 24 measured Miles, trotting most of it, without Pain or Change of Urine; in one Word, I can be as certain of the

dissolving

diſſolving Power of my Medicines as I can be without ſeeing the Diſſolution.

Dr. *Morgan* adviſed me to drink a Pint of Lime-water every Day. Colonel *Morgan* and this Lady coming on a Viſit, adviſed me to take 4 Pills of *Alicant* Soap Morning and Evening; upon which I reſolved to add the Soap-Pills to the Uſe of the Lime-Water; only, inſtead of the Quantity propoſed, I took between 20 and 30 a Day, amounting to near an Ounce; which I thought I might ſafely do, well knowing, that Mrs. *Stephens*'s Preſcription amounted to almoſt 3 Ounces of Soap, beſides other Ingredients: Afterwards I found in an Extract publiſhed in the Magazine, taken from Dr. *Whytt*'s Treatiſe about diſſolving the Stone in human Bodies, the Preſcription of the very Medicine I uſed; only a Quart or three Pints of Lime-Water inſtead of a Pint, upon which I doubled my Quantity.

I have ſince borrow'd the Treatiſe myſelf, and would earneſtly recommend the reading of it to every Perſon troubled with that Diſtemper. The Experience the Doctor has had of the real Effect of this Preſcription in this Diſtemper, join'd with the many Experiments I have found of the diſſolving Power of Lime-Water and Soap, gave me great Satisfaction.

I have uſed with great Succeſs Stone-Lime newly calcin'd; but by thoſe Experiments it ſhould ſeem, that the diſſolving Power of Lime-Water made of Oyſter-Shells is almoſt double to that of Lime-Stone.

There are two good Qualities attending theſe Remedies; the firſt is, That they are cheap, eaſily come at, and prepared by one's ſelf. 2*dly*, That they may be

be safely used for a long time, without Danger to Health, I can vouch by my own Experience; for a Quart of Lime-Water, and an Ounce of Soap, has never given me the least Nauseating, Lowness of Spirits, or Abatement of Appetite, and I think I was never better in Health than I am now.

My Motive for being so particular in this Affair, is a Desire to be instrumental, by your means, of giving Ease to others in so unhappy a Condition; being firmly persuaded, that what has already so far reliev'd me, will, if prescrib'd, dissolve Stones of greater Magnitude than I suppose mine to be. From

Abergavenny, Dec. 1746.

Your affectionate Brother,

Robert Lucas.

X. *The* Figures *of some very extraordinary* calculous Concretions *formed in the* Kidney *of a Woman; communicated by Mr.* Charles Lucas *at* Dublin.

See TAB. III.

Presented March 26. 1747.

PARTS of a *calculous Concretion* formed in the left *Kidney* of *Mary Anne Mac-Mahon*, otherwise *England*, taken out after her Death, in the 30th Year of her Age.

Figure

Figure I. A View of the anterior Part of the *Calculus* in its proper Situation, wanting, to complete its Form, *fig.* III. and V. and ſome other ſmall Pieces which were joined or adhered to it at *A*.

Fig. II. A View of the poſterior Part, completely the reverſe of *fig.* I.

Fig. III. A View of another Portion, which, by the Intermediation of ſome ſmaller Pieces, was joined at *B* to *fig.* I. at *A*.

Fig. IV. The reverſe of *fig.* III.

Fig. V. A Portion which ſeem'd broke off *fig.* I. at *A*; for it fitted it exactly at *C*.

Fig. VI. VII. VIII. Different Fragments, whoſe Places could not be certainly determined.

Fig. IX. A *Nucleus* of a dark Olive-Colour, and oval Figure, of the common Texture and Conſiſtence of ordinary *Calculi*, diſcovered by cutting *fig.* V. tranſverſely at *D*.

Fig. X. A tranſverſe Section of *fig.* II. at *E*, very ſolid, white, and ſemi-pellucid, except at *F*, where a brown Vein, of the Colour of the Surface of the *Nucleus*, *fig.* IX. at *G*, and very porous, runs through it.

XI. *Part of a Letter from Mr.* William Arderon *F. R. S. to Mr.* Henry Baker *F. R. S. concerning the Formation of* Pebbles.

Dear Sir,

Read April 2. 1747.

IN my late Searches after Sands, Pebbles, and other Foſſils, in our County of *Norfolk* (ſome whereof I had the Pleaſure to ſend you not long ago) I made ſuch occaſional Obſervations on the Situation and Condition of the ſeveral Bodies I met with, as Reaſon muſt, I think, ſuggeſt to every Man that conſiders them. I ſhall trouble you with no Hypotheſis, nor form any random Gueſſes, to account for ſuch their Situation, and the Condition wherein they are found; but, if a Relation of true Facts, and Concluſions naturally deducible therefrom, may prove acceptable, they are intirely at your Service.

In all *Strata* of Pebbles, that I have yet examined, there are ſome which are broken, and whoſe Pieces lie together, or very near each other; but, as Bodies of ſuch Hardneſs could not be broken without ſome conſiderable Force or Violence, their Situation implies, that they ſuffered ſuch Force or Violence as broke their Parts aſunder, in or near the Place where they at preſent lie.

Others again have had Pieces broken from them, though not the leaſt Fragment of thoſe Pieces can now be found: From whence we muſt conclude, that whatever might be the Cauſe of their Fracture, they muſt either have been broken at ſome Place

diſtant from where they now lie, or the Pieces broken from them muſt at ſome time or other have been removed to ſome diſtant Place.

Several of theſe Pieces of broken Pebbles have their Edges and Corners ſo very ſharp, that it ſeems as if they had never been removed from the Place where they received the Damage. Others have their Sides and Corners ſo blunted, rounded, and worn away, that one cannot help imagining they muſt have been very roughly toſſed backwards and forwards againſt other hard Bodies, and that too with great Violence, or for a very long Continuance; ſince, without a great deal of Friction, ſuch hard Bodies could ſcarcely have been reduced to the Forms they are now found in.

It may poſſibly be objected, that theſe Pieces of Stones grew in the Figure wherein they now appear; but I am fully ſatisfied, that any Man who will take the Pains to examine theſe Bodies carefully, will ſoon be convinced, from their Veins, or Grain, or Coats, which ſurround each other, ſomewhat like the different Years Growth in Trees, that they muſt once have been complete and intire: And this will be more fully evident, if they are compared with a Stone broken by Art.

Among theſe *Strata* of Pebbles are ſeveral Fragments of various Kinds of Marble, various Kinds of Sand-Stone, and various Kinds of *Gypſum* (though this Part of the Kingdom affordeth no ſuch thing); moſt of which have attained the Hardneſs of the very hardeſt of our Pebbles, as it ſhould ſeem, by lying amongſt them.

Such

Such Pebbles as are found here in *Strata* near the Surface of the Earth, are much more brittle, and break easier without Comparison, than those which lie in deeper *Strata*: For, if the first of these fall, but with their own Weight, upon any other Stone, from the Height of 3 or 4 Feet, they will break very frequently into ten or a dozen Pieces; whereas such as are found deep in the Earth will endure being thrown against one another with all the Force one can give, and that too twenty times perhaps, before the least Splinter of them can be broken off.

I have constantly found, that the more clean and transparent the Sands are with which our Pebbles are mix'd, the more beautiful the Pebbles themselves are, however different their Colours be.

It is wonderful to observe and consider with what amazing Skill the Creator of all Things hath disposed the different *Strata* of the Earth, to serve the Purposes of His Wisdom.

The vegetable Mould or Surface of the Earth is compounded or made up of Sands, Clays, Marls, Loams, rotten Stalks, and Leaves of Herbs, *&c.* serving as a proper Bed and Covering, as well as a Receptacle and Conductor of Moisture, to the Roots of Trees and Plants in general.

Sands and Pebbles may be consider'd as Drains for carrying off the redundant Moisture, to where it may be ready to supply the Place of what is continually rising in Exhalations; but, lest the *Strata* of Sand should be too thick, small ones of Clay are often placed between, and seem intended to prevent this Moisture from departing too far from where it

it may prove of general Uſe. And, leſt theſe curious but thin Partitions of Clay ſhould give Way, by their Softneſs, for the Particles of Sand to inſinuate into them, and thereby let the Moiſture paſs through, thin Cruſts of a ferrugineous Subſtance are placed above and beneath each of theſe clayey *Strata*, and ſerve effectually to keep the Clay and Sand aſunder.

The Obſervations you have now read muſt be underſtood to relate to the County of *Norfolk* only; for I have never had any Opportunity of ſearching the Bowels of the Earth in other Places; but the general Uniformity of Nature makes me ſuppoſe the Situation and Circumſtances of Pebbles, Sands, *&c.* in other Counties may not be very different. Believe me,

SIR,

Norwich, March 3. 1745-6.

Yours, &c.

W. Arderon.

XII. *A Letter from* Arthur Dobbs *Esq*; *of* Castle-Dobbs *in* Ireland, *to the Rev. Mr.* Charles Wetstein, *Chaplain and Secretary to His Royal Highness the Prince of* Wales, *concerning the Distances between* Asia *and* America.

S I R,

Read April 9. 1747.

I AM extremely obliged to you for the Trouble you have taken, in corresponding with Professor *Euler* * upon the *Russian* Discoveries Eastward from *Kamschatka*, and communicating to me the Accounts he had of *Behring*'s last Voyage, and of his Discovery of the Lands North-East of *Japan*; which the Professor could only have inaccurately, not having seen any Journal to fix the Latitude and Longitude of the Countries he then discover'd: But since Professor *Euler*, sway'd by the Opinion of Captain *Behring*, seems still to believe that the last Land he discover'd is joined to *California*, which Country is now known to be Part of the Continent of *America*, and not an Island (in which Fact of its being continuous to *California* I differ still in Opinion from him,) for, if that were a Fact to be depended upon, I would candidly own, that there could be no Passage from the North-West of *Hudson's-Bay* to the Western Ocean of *America*, without sailing near 70° of Longitude; the Distance of the North-East Cape of *Asia* from the North-West of *Hudson's-Bay*, in a Parallel almost as far North as the Polar Circle, before the Passage can

* See *Trans.* N°. 482, p. 421.

can be made to the *Pacific Ocean*; which might therefore be very reasonably call'd an impracticable Passage, as it could not possibly be made in one Summer, if at all); and since Professor *Euler* has been so kind as to give me Captain *Behring*'s Reasons for supporting his Opinion, which are principally from the small Distance he supposed it was, from the Coast he discover'd, to the Western *American* Coast at *California* (which he imagin'd was much nearer his North-East Cape of *Asia* than it is in Fact); I must therefore, in return to the Professor's Goodness, in communicating to me all he has known in that Discovery, beg Leave to give you this further Trouble of communicating to the Professor my Reason for still dissenting from *Behring*'s Opinion, that the Land he discover'd last was Part of the Continent of *America*, or continuous with *California*; and if he find the Reasons for supporting my Opinion make it more probable, that there still may be a large Opening betwixt these new-discover'd Countries and *California*, I am sensible it will give the ingenious and learned Professor great Pleasure, to think we may yet hope for a Passage by *Hudson's-Bay* to the Western *American* Ocean, without being obstructed with Ice after passing *Hudson's-Sreight*.

The Professor imagines I might have been led astray, by not considering, that the North-East Cape of *Asia* is much more Easterly than has been laid down in any former Charts; which is now known accurately, by the Eclipse of the Moon observed by Captain *Behring* at *Kamschatka*.

I have an Abstract of his Journal by me, upon his first Discovery in 1728, and 1729, when he observed that Eclipse, and the Calculation of the Longitude

from it; and ſtand by his Longitude he has fix'd; and allow that his North-Eaſt Cape is in the other Hemiſphere; reckoning Eaſtward, either from *Fero*, as the firſt Meridian, or from *London*; which laſt I ſhall follow.

Behring fixes his North-Eaſt Cape 126° 7′ Eaſt Longitude from *Tobolski*; and *Tobolski* is 86° Eaſt from *Fero*; ſo the Cape is 212° 7′ Eaſt of *Fero*, or about 194° Eaſt from *London*. —— By Captain *Middleton*'s Obſervation of *Jupiter*'s *Satellite* at *Churchill* River in *Hudſon's Bay*, that River is 95° Weſt from *London*; which, added to 194°, makes 289°; conſequently the North-Eaſt Cape of *Aſia* is 71° diſtant from *Churchill*, to complete 360°; which, in the Latitude of 65°, computing 8 Leagues to a Degree of Longiude, of which 20 make a Degree of Latitude, the Diſtance betwixt that Cape and *Hudſon's Bay* would be 568 ſuch Leagues.

From the known Longitude of the North Cape of *Japon* in 40° Latitude, which is pretty exactly known, from the Obſervations made by the Jeſuits at *Peking*, and is about 150° Eaſt from *London*, and from the beſt computed Longitude of *California* in 40° North Latitude, it lies in 130° Longitude Weſt from *London*, making together 280°, leaves 80° for the Diſtance of *California* from *Japon*; allowing 17 Leagues to a Degree of Longitude in 40° North Latitude, the Diſtance would be about 1360 Leagues: By the ſame Calculation *California* muſt be at leaſt 7 or 800 ſuch Leagues from the North-Eaſt Cape of *Aſia*; ſo that, in ſo great a Space there may be very great Countries or Iſlands *, without ſuppoſing the new diſcover'd Country continuous to *California*, and might well allow of an open Chanel or Sea, from 50 to 100 Leagues wide, between the diſcover'd Coaſt and *California*.

By

By the Account given to Professor *Euler*, *Behring* sailed Southwardly to the Isles of *Japon*, and from thence sailed Eastwardly 50 *German* Miles, about 250 *English* Miles; which makes about 80 Leagues, of 20 to a Degree. At that Distance from *Japon* he discover'd Land, which he coasted North-West; still approaching towards the North-East Cape, without going ashore, until he came to the Entrance of a great River; where sending his Boats and Men ashore, they never return'd, being either lost, kill'd, or detain'd by the Natives, which made his Discovery incomplete; his Ship being stranded, and he afterwards died in an uninhabited Island.

As no Latitudes nor Longitudes are fixed by this Account, I must believe he sailed from *Kamschatka* South-East, perhaps more Southerly than to 50° Latitude; and there found Land North-East from *Japon*; otherwise, by coasting it North-West, he could never approach the North-East Cape, which is, at least, 40° Longitude East of *Japon*; and if he made Land 80 Leagues East of *Japon*, he must have sailed North-East to make the North-East Cape. I have therefore Reason to believe this Coast was Part of that he saw in his first Voyage, where he lost his Anchor; and is the Coast *Gama* discover'd, and the *Dutch* afterwards called the *Company's Land*, East of the Streights of *Uzicez*, which is at least 7 or 800 Leagues

* The *Japonese*, in their Maps of the World printed in *Japan*, have laid down in this very Tract two Islands as large as *Ireland*, with the Names to them, as appears in that Map bought by Dr. *Kempfer* in *Japon* in 1686; now in Sir *Hans Sloane*'s Museum.

C. M.

Leagues West of any known Land of *America*, and above 1000 near the Latitude of *Japon*: So that, if I should allow 700 Leagues for Countries or Islands East of his new-discover'd Coast, there might still be a Passage of 100 Leagues for the Southern or *Pacific Ocean* to communicate with *Hudson's Bay*, and to cause such great Tides and Currents, as are found on the North-West of *Hudson's-Bay*; as also a free Passage for the Whales, which are seen in all the Openings North-West of that Bay, and are caught there in Numbers by the *Eskemaux* Savages: For, as these don't go in by *Hudson's Streight* from our *Atlantic Ocean*, it cannot be presumed that they should go up by *Japon* towards the North-East Cape, and from thence go 70°, or above 560 Leagues, to *Hudson's Bay*, and be there in the Month of *June*, and, after staying until *September*, return again the same Way to the Southern Ocean, to pass the Winter.— Now, as *Behring* only coasted at a Distance, he could not possibly know whether it was a Continent, or great Island; the last of which seems the most probable: However, a few Months now, if our Ships return safe, will give us a Certainty on one Side or the other; altho' I am sanguine enough to believe they have by this time sailed through, and discover'd this so much wish'd for Passage.

These, Sir, are the Reasons I have still to expect Success in the Attempt I have promoted; and, if you think it may give any Satisfaction to Professor *Euler* to know the Reasons that support my Belief of a practicable safe Passage, be pleased to communicate it to him, with my Compliments for the Trouble I

 have

have given him by you, and accept of my best Acknowledgments for your Favours. I am, with the greatest Regard and Esteem,

SIR,

Castle-Dobbs, Feb. 10. 1746-7.

Your most obedient

humble Servant,

Arthur Dobbs.

XIII. *A Letter from the Rev. Mr.* G. Costard, *to the Rev.* Thomas Shaw, *D. D. F. R. S. and Principal of* St. Edmund-Hall, *concerning the* Chinese Chronology *and* Astronomy.

Reverend Sir,

Read April 30. 1747.

THE Subject of our late Conversation turn'd upon the Affectation of some Nations, in carrying up their Histories to so immoderate a Height, as plainly to shew those Accounts to be fictitious and without Foundation. This, it was agreed, was the Case of the *Babylonian* and *Egyptian* Accounts; and you seem'd to think it would be found to be the same with any other People that should make the like Pretensions.

The only People in later Times that have been thought to contradict this Opinion are the *Chinese*, of whose History the World hath been taught

taught to entertain very extraordinary Conceptions. But that even They will be no Exception to your Surmize, but, on the contrary, a ſtrong Confirmation of it, will, I perſuade myſelf, appear, from what I am now going to offer.

I need not inform you, that the Eaſtern Writers in general are much addicted to Fable and Romance. This is a Fact too well known to need any Proof; and therefore great Judgment is many times required to diſtinguiſh what is real from what is purely imaginary, improbable, and abſurd. I ſay this, not ſo much with regard to their Accounts of foreign Nations, with whoſe Affairs they may be preſumed to be leſs acquainted, as of their own ancient State and Condition, and that in Ages not exceedingly remote. But if this Obſervation holds but too true, with reſpect to thoſe whoſe Hiſtory we are in ſome manner acquainted with, how much ought it to put us upon our Guard as to thoſe we are in great meaſure abſolute Strangers to?

The beſt Accounts we have received of *China* are owing to the *Jeſuits*. But thoſe Accounts themſelves are, I am afraid, to be frequently received with great Caution. Theſe Fathers have been ſometimes, perhaps, not ſufficiently verſed in *European* or *Chineſe* Learning, or both, to give us proper Information. At other times, it may be, they have been too much prejudiced in Favour of their Converts, or had Ends to ſerve, of which the World hath not been properly enough appriſed. To have propagated their Religion only in a barbarous and uncultivated Nation, would not have been ſo much for the Credit of the Miſſion, as to have been able to introduce it

among a People civilized and poliſhed by Arts and Literature.

Suſpicious as theſe and the like Circumſtances are, is it not ſurpriſing to hear Authors, upon their Words only, and upon little or no Foundation beſides (as I queſtion not will appear), aſſerting with ſo much Poſitiveneſs, that the *Chineſe Hiſtory reaches up indiſputably to the Times of Noah* (*a*)? A thing ſo far from being *indiſputably* true, that no Article whatever perhaps will admit of greater Debate. 'Tis true indeed, the *Chineſe* give us a long Liſt of Kings that reigned among them from the Time of *Fo-hi*, and a Series of Dates, that, if allowed, may carry up his Age 2952 or 2847 Years before the Chriſtian *Æra* (*b*). But how eaſy is it to feign * Dates and Succeſſions of Kings! Let it be made appear what Foundation this Chronology depends on; what ancient Monuments the *Chineſe* have, and in what manner preſerv'd. Marbles, I ſuppoſe, they have none; and their Paper, ſuch at leaſt as is brought into *Europe*, appears to be of too fine a Conſiſtence for the Preſervation of Records.

You will be told, Sir, perhaps, that a great Part, at leaſt, of their *Chronology* is verified by *Eclipſes*. A very pompous Argument! but, when narrowly examined into, will be found to prove juſt nothing at all. We are told indeed (*c*), that the ancient *Chineſe* Obſervations conſiſt of 26 *Eclipſes* of the *Sun*, and 21 Conjunctions of *Jupiter* with the fix'd Stars.

The

(*a*) *Shuckford's* Connect. Vol. I. p. 101. (*b*) *Ibid.* p. 29.

* See theſe *Tranſ.* N°. 415. p. 397, where this Chronology ſeems to be ſet in a true Light by the Viceroy himſelf of *Canton* 1724. *C.M.*

(*c*) *Obſ. Math. Aſtron. Geogr. Chronol.* Tom. I. *Pref.* p. 13, 14.

The oldest *Eclipse* of the Sun is placed in the first Year of the Reign of *Tching-Cham* (d), supposed to coincide with the Year before *Christ* 2155: But the oldest Conjunction of *Jupiter* reaches no higher than the Year after *Christ* 73 (d): And how inaccurate the Observation was, appears from hence, that the *Chinese* only mark the Day when that Conjunction happened.

But the Question naturally arising here is, How it comes to pass, that the *Chinese* Accounts afford no Example of any planetary Conjunction before this, when they produce an *Eclipse* of the Sun 2228 Years earlier? By what good Fortune came that *Eclipse* to be preserved, and all Appulses of the *Planets* to fixed Stars for so many Years be lost? Let us suppose, that these were Things below the Notice of *Chinese* Astronomers; or that they did not know what Use to make of them. But in what manner must we account for this, That we hear nothing of any other *Eclipse*, till the Year before *Christ* 776 (f)? That all the *Eclipses*, observed during so long an Interval as 1379 Years, should have perish'd, and this *one* have escaped, requires a pretty strong Faith to believe.

But farther, we are told, that they observed the *Winter Solstice* in the Year before *Christ* 1111. There is nothing, it is true, impossible in this; for it is not said how accurate the Observation was. The Difficulty

(d) *Ibid.* p. 18. (e) *Ibid.* p. 15.

(f) Not much before the oldest *Babylonian* Eclipse that is preserved. *See Letter to* M. Folkes *Esq*; p. 21.

Difficulty is only to aſcertain the Fact, and convince reaſonable People that it was made at all.

'Tis well known, and allow'd by the Miſſionaries themſelves, that the Reception they have met with in *China* hath been more owing to their Character as *Philoſophers* than *Apoſtles (g)*. When therefore they brought with them into the Country Accounts of *European* Diſcoveries, and particularly in *Aſtronomy*, might not the *Chineſe*, agreeable to their vain-glorious Character, tell them, that they had of their own much older than any thing they could pretend to? It may be ſaid indeed, that this is no more than Suppoſition, and which conſequently argues but little: But then the Suppoſition is ſo eaſy and natural, that it requires at leaſt the contrary to be made out by ſome very good Proof.

One Reaſon why this may be inſiſted on the more is, that the *Chineſe*, according to the *Fathers* themſelves, have not always been faithful in their relating Obſervations. *Y-hang*, about the Year after *Chriſt* 721, had the Reputation among them of an able *Aſtronomer*; but being miſtaken, it ſeems, in his Calculation of an *Eclipſe*, rather than own his Ignorance, he pretended, that the *heavenly* Bodies did not always obſerve the ſame Laws. In Support of which extraordinary Hypotheſis, he urged, that, in the Time of *Tſin* (*h*), the Star *Sirius* was eclipſed by the Planet *Venus*; tho' the Latitude of *Sirius* is 39° 32′ 8″, and that of *Venus* never exceeds 4°. The ſame

(g) *Obſervat. ut ſup.* Tom. 2. *p.* 117. (h) *Obſervat. ut ſup.* Tom. 2. p. 86. *Flamſtead's* British *Catal. Greg. Aſtron.* p. 5.

ſame Sort of Obſervation with this, I ſuppoſe, is the other of the Conjunction of *Saturn*, *Jupiter*, *Mars*, *Venus*, and *Mercury*, in the Conſtellation *Che*; when the *Sun* and *Moon* likewiſe were in Conjunction in 15° of *Aquarius*, in the Time of *Tchouen yu* (*i*).

And to put it out of all Doubt, that the *Chineſe* are capable of obtruding upon the World *fictitious* Obſervations, we need no other Authority ſtill than that of the *Learned Fathers* themſelves. In the Year 1725 (*k*) the *Miſſionaries* ſent into *Europe* an Account of an Approximation of the four Planets *Jupiter*, *Mars*, *Venus*, and *Mercury*. Such planetary Conjunctions, it ſeems, in *China*, are look'd on as happy Omens of good Fortune to the Prince upon the Throne. The *Chineſe* therefore, as if bred up at the Court of *Verſailles*, with a true *French* Politeneſs, in Compliment to their Sovereign, mark'd in their Regiſters a Conjunction of all the 7. This falſe Account of an imaginary Conjunction, as the *Learned Jeſuit* himſelf obſerves, may, in future Times, be the Occaſion of very great Errors. ——To the *Chineſe*, I ſuppoſe he means; for in *Europe* the Danger will be but ſmall; where there are better Tables, exacter Accounts, and more accurate Obſervers, than the moſt ſanguine *Jeſuit* will pretend to be among the *Chineſe*. But if they would venture at recording ſuch a ſpurious Obſervation, at a time when they were ſure of being detected, what may we not ſuſpect them to

(*i*) *Obſervat. ut ſup.* Tom. 2. p. 149. (*k*) *Obſervat. ut ſup.* Tom. 2. p. 33.

to have been guilty of, when they had none to confront them; and how little may we presume they know of the Uses to be made of *Celestial* Observations?

We are told, with great Pomp and Assurance *(l)*, That there *always* was in *China* an Office of *Mathematics*, and another of History: That it is the Business of the former to calculate *Eclipses*; and of the latter to register them, and every other Occurrence that happens in the State.

It would have been well, if the *learned Fathers* had told us with a little more Exactness what we are to understand by the Term *always*; and whether the *Chinese* are acquainted enough with the Uses for which *Eclipses* serve, to make it probable, that they should have had such an Office any *considerable Number of Years*, and much less *always*. May we not in the mean time suspect, upon hearing such Language as this, that the *Fathers* mistake *Pekin* for *Paris*; and, having their Heads full of the *Academy of Sciences*, cannot help figuring to themselves the like in the remotest Corners of *Asia*?

Be this as it will, they tell us, That the *Mathematicians* have often had the Credit to take out of the Registers their *false* Calculations, and substitute in their room others, agreeing with Observations. — But where Things are kept with so little Exactness, what can be expected but Confusion? What less indeed can be expected than what the *Fathers* assure us themselves hath frequently been the Case *(m)*, That, by

(l) Observat. ut sup. Tom. 2. p. 158. *(m) Ut sup.* p. 159.

by this means *Eclipses* have been preserved, that Calculations made by *European* Tables demonstrate to be false? Can we wonder likewise, that the *learned Fathers* should doubt many times, whether such or such a particular Eclipse be an *Observation* made at the Time, or the Result only of a *Calculation*, and perhaps a false one (*n*)?

For, after all that hath been said of *Chinese Eclipses*, and the *Calculations* of them, it is agreed, (*o*) that, before the Time of *Lieou hong*, or *A. D.* 206. they had no fix'd Principles upon which to proceed in that Business. — This Observation, I am afraid, will extend to much later Times; or 'tis scarce to be imagined they should look upon total *Eclipses* of the Sun as ill Omens.—In consequence of this superstitious Belief, we are told (*p*), That the *Chinese Astronomers* have carried their Compliment to the Family on the Throne so far, as to affirm no such *Eclipse* could happen during their Time. On the other hand, should an *Eclipse* of this sort happen, without being foretold, they immediately pretend it to be a Warning from Heaven of some Misfortune likely soon to befal the Government. But if it should be foretold, and not come to pass, they would then make the many Virtues of the Sovereign the preventing Cause; and, what is better still, a Shelter for their own Ignorance. Such Notions as these however, I think, plainly demonstrate them to be very *bungling Astronomers*; and that they can hardly look upon these *Phænomena* as

(*n*) *Obs. ut sup.* Tom. 2. p. 159. (*o*) *Observat. ut sup.* Tom. 2. p. 32. (*p*) *Ibid.* p. 33.

as depending on establish'd invariable Laws of Nature; the Consequence of which is, that they can no more attempt bringing them to a *Calculus*, than Winds, Thunder, and Lightning, and the like.

It was observed before, that the *Mathematicians* had many times the Art or the Credit to take out of the Registers their false Calculations; but we are told in the same Place, That, before they were reposited there, they were presented to the Emperor, for his Inspection. Let any one, that knows the least of the despotic Governments of the East, reflect on the Probability of this; and whether the Attempter would not run great Danger of paying for it with his Head.

What hath been already said, will, I suppose, be more than sufficient to shake the Credit of *Chinese* Observations. But what must we think of those very ancient ones, when we are farther told (*q*), That, from the Time of *Tchun-tsieou*, or 480 Years before *Christ*, the *Chinese* themselves allow *Astronomy* was almost intirely neglected among them; and that *Tsin-chi-hoang*, whose Reign began in the Year before *Christ* 246, order'd all Books of *History* and *Astronomy* to be burned (*r*)? But every one will easily imagine what Destruction of Observations must have been made during a Neglect of these Studies for 234 Years; and how little would remain to be burned by this ***anti-astronomical Prince.***

It

(*q*) *Observat. ut sup.* Tom. 2. p. 1.   (*r*) *Ibid.* p. 2.

It was owing to this Devaſtation, it ſeems, that the *Chineſe* are ſaid to have loſt the Method taught by the Ancients, and particularly the Emperor *Yao*, of calculating the Places of the 7 *Planets*, and the fixed Stars (*ſ*). It may with good Reaſon be queſtioned, whether they really had any Methods of calculating their Places at all: For to what Purpoſe could ſuch Calculations ſerve, when their Catalogues of Stars, many Centuries later, are acknowleged to have contained nothing more than bare *Names*, without *Latitudes*, *Longitudes*, *Right Aſcenſions*, or *Declinations?* Such their Catalogues were, if they deſerve that Name, that were made under the Race of Emperors called *Song* (*t*), or between *A. D.* 591 and 620; and it will be difficult to prove they were any thing elſe, before the *Jeſuits* introduced there *Tycho-Brahe*'s, or other *European* ones. As to the Places of the Planets, how little they were able to compute them will appear from hence, that the utmoſt that *Lieou-hin* and *Lo-hia-hong*, in the Year before *Chriſt* 66, pretended to, was to calculate a plain rectangled Triangle (*u*). In what manner they did this is not ſaid; but it would be well, if the *learned Fathers* would make it appear that the *Chineſe* had, in much later Times, any thing like a Table of natural Sines and Tangents. A ſmall Skill in *Mathematics* is requiſite to apprehend from hence how bungling their Aſtronomy muſt have been; and if ſo, much more that of the Ages preceding them! And

(*ſ*) *Obſervat.* &c. Tom. p. 3. (*t*) *Obſervat.* &c. Tom. p. 65. (*u*) *Obſerv. ut ſup.* Tom. 2. p. 8.

And it will be yet further hard to imagine that they knew how to find the Places of the Planets, when we are assured (*x*), that *Tchang-tse-tsin*, about *A.D.* 550, was the first Person that introduced Equations into their Computations of the Planets Motions; that *Co-chiou-king*, about *A. D.* 1280, was the first *Chinese* that knew any thing of *Spherical Trigonometry* (*y*); and that, before the Arrival of the *Jesuits*, they were intire Strangers to the Inclinations of the Planes of the planetary Orbits (*z*).

After what hath been said, I think we need but little more to convince us of the small Acquaintance of the *Chinese* with *Astronomy*. They tell us however themselves (*a*), That, from the Time of *Tsin-chi-hoang* above-mention'd, they had no expert *Astronomer*, no Books of *Astaonomy*, nor known Method of computing. All that remained were some *confused Traditions*, *Catalogues* of *Stars* and *Constellations*, and *Fragments* of *Books*. A mighty Encouragement all this, to expect reforming the *European Astronomy* or *Chronology* by the *Chinese*! What sort of *Catalogues* these were, hath been already observed; and we may, without Offence, I presume, beg to be excused from paying over much Deference to *Chinese Tradition*; at least, till the Fathers have better determined what Degree of Assent it deserves.

About *A. D.* 164. several *Jew* Families, and other Subjects of the Western Empire, came into *China* (*b*). At that time, as it is allowed by the *Jesuits*

(*x*) *Observat. &c.* Tom. 2. p. 58, 59. (*y*) *Ibid.* p. 114.
(*z*) *Ibid.* p. 84. (*a*) *Observ. ut sup.* Tom. 2. p. 3.
(*b*) *Observ. ut sup.* Tom. 2. p. 119.

Jesuits themselves, *Ptolemy's Astronomy* was in great Vogue all over the East; and they seem to suspect, that by this means the *Chinese* might get some faint Knowlege of it. It is certain, that from this time we meet with Things unknown to their former Accounts of *Astronomy*. At this time, it is said (*c*), *Tchang-heng* made a Catalogue of 2500 Stars. Such a Catalogue as those already mention'd were, we may suppose it to be, if it was at all; for *Tchang-heng's* Book is lost; and what Accounts we have of him or his Works, depend on the Authority of others.

A. D. 284. (*d*) liv'd *Kiang-ki*, the first *Chinese* that is said to have known any thing of the Motion of the fix'd Stars. This we see was 120 Years after the above-mention'd Arrival of the *Jews* in *China*; but either they must have been unskilful Relaters, or the *Chinese* bad Disciples, since *Kiang-ki*, it seems (*e*), made that Motion to be at the Rate of 1° in 50 Years; whereas *Ptolemy*, it is well known, made it 1° in 100 Years. It may be said indeed, that this Difference shews it could not be borrowed from *Ptolemy*; but then it shews at the same time, that it could not be the Result of any Series of Observations; and that is as much, I think, as we need be concerned about. And this will appear yet farther, by remarking, that *A. D.* 460, it was made by *Tson-chong* 1° in 45 *solar* Years and 9 *lunar* Months (*f*). At other times it was made yet different still; but never, I think, from Observations of the Stars themselves.

(*c*) *Obs. ut sup.* Tom. 2. p. 25. (*d*) *Ibid.* p. 44. (*e*) *Ibid.* (*f*) *Ibid.* p. 52.

felves. The Method, it feems, was by comparing the Places of the Solftices in their own Time, with their Places in the Reign of the Emperor *Yao* (*g*), whom they fuppofed to have lived at a Time co-inciding with 2300 Years before *Chrift*. Let us fuppofe them to have been right in this (a Thing we are by no means obliged to allow), yet, as they never appear capable of taking the *Solftices* with any tolerable Degree of Accuracy, we cannot wonder at any Miftakes we may meet with.

You have feen, Sir, all along, conftant Mention made of *Chinefe Calculations*; the very Word ufed by the Fathers themfelves; tho' I am fenfible that Term will be apt to lead the unwary into great Errors. For the moft that can be made of their *Calculations* is nothing more than finding the Places of the Luminaries by Numbers (expreffing their Periods and Parts of Periods), or, in other Words, by their mean Motions. For, as to the *Sun*, we are affured (*h*), they made his Motion one *Chinefe* Degree in a Day, without knowing any thing of an *Equation* neceffary to correct it. It was not till the third Century that they had any *Equation* for the *Moon* (*i*); and *Tchang-tfe-tfin*, about *A. D.* 550, as we have feen, was the firft that ufed any for the *Planets*. It is obfervable, however, with regard to this Author, that there are no Writings of his extant (*k*); and therefore what is here faid of him, may be nothing more than Report. Among an idle vain

(*g*) *Obferv. ut fup.* Tom. 2. p. 148. (*h*) *Ibid.* p. 6.
(*i*) *Ibid.* p. 24. (*k*) *Ibid.* p. 58, 59.

vain People, unacquainted with critical Learning, round Aſſertions paſs for Proof; and in this manner, beyond Queſtion, the *Chineſe* have frequently impoſed upon credulous unskilful *Europeans*.

A. D. 618. began the Reign of the Emperors of the Race of *Tang*; and about this time other weſtern Strangers came into *China* (*l*). From them therefore the *Chineſe* might learn what farther Improvements we may chance to meet with in their *Aſtronomy*, beſides thoſe for which they are aſſuredly indebted to the *Jeſuits*.

It was obſerved but now, that in computing the Places of the heavenly Bodies, the *Chineſe* at beſt knew nothing but their mean Motions: But in ſuch Computations it is neceſſary to begin from ſome *Radix*, or other: *European* Tables generally begin with the Commencement of the Chriſtian *Æra*; the *Chineſe* appear never to have known any. One *Epoch* indeed they have, but intirely imaginary, called *Chang-Yuen* (*m*), and which began ſome time or other at *Midnight*, at the *Moment* of the *Winter Solſtice*, when the *Sun*, *Moon*, and 5 Planets, *Saturn*, *Jupiter*, *Mars*, *Mercury*, and *Venus*, were all in Conjunction, and the *Moon* without any Latitude.

This extraordinary *Epoch* began, according to theſe able Aſtronomers, 143127 Years before the *Winter Solſtice*, in the Year before *Chriſt* 104 (*n*). What time this *Epoch* came firſt into Uſe is not known; but the *Fathers* think, and it muſt be acknowleged with

(*l*) *Obſ. ut ſup.* Tom. 2. p. 71, 72, 96. (*m*) *Ibid.* p. 16.
(*n*) *Ibid.*

with great *Probability*, that it is not older than the burning the Books under *Tſin-chi-hoang* (o), or, as we have already ſeen, the Year before *Chriſt* 246. Should we place it however many Years later, or ſay that it never ſerved any real Uſe at all, we may perhaps come much nearer the Truth.

For the *Chineſe* Aſtronomers, as the *Fathers* obſerve (p), have ſpent an infinite deal of Time and Pains, in ſearching out this *Chang-Yuen*; and which has been carried up, they ſay, by ſome *two*, by others *three* Millions of Years beyond the Time it was above fixed at. But this ſhews to a Demonſtration, that it is an *Epoch* purely fictitious; that, if it was real, it could only be of an *aſtronomical* Nature; and that they muſt be *ſottiſhly ſtupid*, that ſhould from thence collect, that the *Chineſe* had any hiſtorical Memoirs of ſo ancient a Date. For the *Fathers* themſelves allow, that the Opinion which aſcribes to the World a Duration of ſome Millions of Years, is neither the general Opinion of the *Chineſe*, nor of any ancient ſtanding among them (q).

From what has been here offer'd, I think it is pretty evident, that, how ingenious ſoever the *Chineſe* may be in Works of Art, their Talents do not lie towards *Mathematics* and *Aſtronomy*: For, was not this the Caſe, muſt it not be ſurpriſing, that having, as they ſay, ſo long a Series of Obſervations in the one Science, and of Profeſſors in the other, they ſhould never have been able to get beyond the firſt Elements of either?

It

(o) *Obſ. ubi ſup.* Tom. 2. p. 18. (p) *Ibid.* p. 17. (q) *Ibid.* p. 17.

It is not my Deſign to enter into any Controverſy with the *Learned Fathers* of the *Society* of *Jeſus*; the World hath been frequently indebted to them for their Philoſophical Labours; and will be ſo again, when they ſhall have conſidered the *Chineſe* Hiſtory with proper Accuracy, and told us in what Manner they have been able to preſerve Accounts and Obſervations of ſo ancient a Date. Public Libraries, it is allowed (*r*), they have none; nor doth it appear they ever had. Where then could Things ſo uſeleſs, as the Generality muſt have thought *aſtronomical Obſervations*, be repoſited? When intruſted to private Hands, they muſt have run great Riſque of being deſtroy'd by Wars, by Fires, and in popular Commotions; which muſt frequently have happened in ſo long a Courſe of Years.

Let us ſuppoſe Things of this ſort are of more Value to the *Chineſe Commonalty*, or, if you pleaſe, their *Nobility*, than they are to the *European*; and that they would lay Hands on every thing they could meet with of the kind; and, when once in their Poſſeſſion, would preſerve it with a religious Exactneſs: But whence then comes it to paſs, that it is *ſo difficult a Matter* in *China* to meet with Books upon theſe Subjects (*ſ*), to underſtand them when found, or to get any Aſſiſtance from the Natives towards underſtanding them?

But beſides, are not Writings thus kept in private Perſons Cuſtody, unleſs carefully laid by, apt to be ſcribbled

(*r*) *Obſerv. ut ſup.* Tom. 2. Pref. (*ſ*) *Ibid.*

ſcribbled on by the Scioliſts; ſo that it may be hard to diſtinguiſh many times the Text from what may be called the Comment? Is not this, in Fact, according to the *Learned Fathers* own Acconnt (*t*), very frequently the Caſe? May not, by this means, a *Calculation*, if it muſt be ſo called, be miſtook for an *Obſervation* made many hundred Years before? It is confeſſed (*u*), that *Martini* was impoſed upon in this very manner; and it is much to be ſuſpected that he hath not been the only one.

You ſee, Sir, that I have produced the *Jeſuits* own Authority for every thing here offered. I deſigned to have cited their Words at Length, but that I found would have ſwell'd theſe Papers above the Size of a Letter. I am not conſcious to myſelf of having miſrepreſented them; I am ſure I have not done it wilfully; for I have nothing in my View but Truth. I am,

SIR,

March 2.
1747.

Your moſt obedient

humble Servant,

G. Coſtard.

P. S. The Title of the Work I have here cited, is, at length, *Obſervations Mathématiques, Aſtronomiques*,

(*t*) *Obſ. ut ſup.* Tom. 2. Pref.

(*u*) *Obſ. ut ſup.* Tom. 2. p. 103.

miques, Géographiques, Chronologiques, & Physiques, tirées des Anciens Livres Chinois, ou faites nouvellement aux Indes, à la Chine et ailleurs, par les Peres de la Compagnie de Jesu. It consists of three Volumes, printed at *Paris,* 1729, 1732.

XIV. *Part of a Letter from Mr.* Turberville Needham *to* James Parsons *M. D. F. R. S. of a new* Mirror, *which burns at* 66 *Feet Distance, invented by M.* de Buffon *F. R. S. and Member of the* Royal Academy *of* Sciences *at* Paris.

Dear Sir,

Read April 30. 1747. YOURS of *December* came so late to hand, that I could not answer it till this Occasion. **** I have been at the King's Garden, and am just returned: I there learned, that this Morning they have been trying some Experiments with a new-constructed reflecting Mirror or Mirrors with Success: I knew indeed some time ago, that they had been upon the Design; and M. *de Buffon* had acquainted me with the theoretical Part of the Whole. I had even seen a Part of it executed; but as they had not then essayed it, I would take no notice of it: In one Word, it is *Archimedes* revived; and the Credit of Antiquity, in this Point, is

in ſome meaſure re-eſtabliſhed. This Machine, for ſo I muſt call it, conſiſts of 140 ſmall plain Mirrors, each of about 4 by 3 Inches ſquare; they are fixed at about a Quarter of an Inch Diſtance from each other, upon a large wooden Frame about 6 Feet ſquare, ſtrengthened with many croſs Bars of Wood for the mounting of theſe Mirrors. Each of them have three moveable Screws, which the Operator commands from behind, ſo contrived, that the Mirror can be inclined to any Angle in any Direction that meets the Sun; and by this means the ſolar Image of each Mirror is made to coincide with all the reſt.

There are in all, as I told you, 140 Mirrors; but they tried the Experiment this Morning with 24 only; for ſo many, and no more, were then ready for the Purpoſe: The Effect was, that, in very few Seconds of Time, a combuſtible Matter they had prepared with Pitch and Tow, daubed upon a Deal-Board, was ſet on Fire, and burn'd vigorouſly at the Diſtance of threeſcore and ſix *French* Feet. Judge now of the Effect 140 will produce; and whether the Invention may not be improved to the Height of all that has been advanced of *Archimedes* by the Ancients. The only Difficulty they found was, to make the ſolar Images of the Mirrors coincide; but this is owing to the yet Imperfection of their Method of mounting, which may be eaſily improved.

The Dimenſions I have given in of the Mirrors and Frame were only gueſſed at from View, for I have not meaſur'd them; ſo you muſt not expect they will ſquare or tally mathematically in the utmoſt Rigour. Nor indeed did I think it neceſſary to

4

to do any more; for the Dimensions of themselves are purely arbitrary. If you have a clear Idea of the Construction of the Machine by this general Description, it is all I have aimed at.

XV. *Extract of a Letter from the Marquis* Nicolini *F.R.S. to the* President, *concerning the same* Mirror *burning at* 150 *Feet Distance.*

SIR,

* * *

Read April 30. 1747. YOU know that the Affair of *Archimedes* setting the *Roman* Fleet on Fire by means of Burning-Glasses, has been look'd upon as a Thing impossible and romantic. *Descartes* positively denied the Fact, which had been believed for so many Ages; and our modern Philosophers, after many Trials, and various Reasonings, have been of the same Opinion. But M. *de Buffon*, being asked if it might be possible to invent a *Phaometer*, or *Machine* for measuring the *Intensity* of *Light*, hath discovered by Trial, that Light was able to produce great Effects in a *Focus* at a great Distance, if one made use of a great Number of Disks, which would reflect so many Images of the Sun, and fling them all into one Place. He put together therefore a sort of *Polyedron*, consisting of 168 small Mirrors, or flat Pieces of Looking-glass, each 6 Inches square; by means of which, with the faint Rays of the Sun, in the Month of *March*, he set on Fire some Boards

of Beech Wood at 150 Feet Diſtance. By increaſing the Numbers of Mirrors, he hopes to be able to do the ſame 900 Feet off.

His Machine has beſides, the Conveniency of burning downwards or horizontally, as one pleaſes; and it burns either in its greater *Focus*, or in any nearer Interval, which our commonly known Burning Glaſſes have not, their *Focus* being fix'd and determined.

Perhaps this Machine may afford a Manner of meaſuring either Light, or the different Degrees of Heat of burning Bodies. The Difficulty is to find the Method of marking the Degrees, and of fixing a Point of Compariſon; for the Point of kindling will not determine it; becauſe that chiefly depends upon the greater or leſs Degree of Inflammability of different combuſtible Bodies.

Mr. *Maupertuis*, in a Letter to the *Preſident*, dated at *Potzdam*, *May* 20. 1747. ſays, that his Friend *Buffon* has recover'd the Burning-Glaſſes of *Archimedes*; that with 168 plane Glaſſes, each 6 Inches ſquare, he has melted a ſilver Plate, at the Diſtance of 60 Feet, and fired pitch'd Boards at 150. Each *Speculum* is moveable, ſo as, by the Help of 3 Screws, to be ſet to a proper Inclination for directing the Rays towards any given Point.

XVI. *Epistola* Jo. Henrici Winkleri, *Prof.* Lipſ. & *R. S.* Lond. *S. ad* Societatem Regalem Londinenſem *data, quæ continet Deſcriptionem & Figuras* Pyrorgani *ſui* Electrici.

Præſes Illuſtriſſime,
Viri Illuſtriſſimi,

Read May 7. 1747.

FACTA in Societatem veſtram ſplendidiſſimam mei cooptatione adeo præclarum munus mihi impertiviſtis, ut in agendis ejus partibus omnem vim ac operam adhibendam mihi eſſe videam. Cum enim ex veſtra Societate per quamplurimos annos opera et ſcripta prodierint, quibus et propter rerum dignitatem, et propter auctorum celebritatem, et propter doctrinæ excellentiam, memoria perpetua conſtabit; facile intelligo quam arduum opus ſit, quod inter viros, quorum fama univerſum literarum orbem peragravit, mihi ſuſcipiendum eſt. Attamen vires animus colligit. Cum enim duobus abhinc annis ex primo meo de electricitatis natura libello ſcintillularum electricarum, quibus ſpiritus vini incenditur, præcipua phænomena in tranſactiones veſtras deſcripta inferri curaveritis, Viri Illuſtriſſimi; in eam adductus ſum opinionem, fore, ut futura etiam opera mea, in qua continuanda non deſiſtam, aliquid efficiatur, quod non diſpliceat. In præſenti vobis, Illuſtriſſime Præſes, Viri Illuſtriſſimi, pro eximio, quo me ornâſtis, beneficio grates ago immortales, meque, quoad potero, exquirendis naturæ

viribus

viribus ſtudium impenſurum eſſe indefeſſum, et, ſi mihi obvenerit, quod notatu dignum videatur, hoc ad vos perſcripturum eſſe polliceor. In libello *Germanico*, in quo anno ſuperiori experimentum *Muſchenbroekianum* explicaturus virium electricarum augmentum in vaſis vitreis deſcripſi, § 30, organi alicujus, quo plures ſcintillæ ſimul oriundæ crepitant, mentionem injeci.

Hujus *Pyrorgani Electrici*, ut voco, imaginem, in figuris æneis dicti libelli non delineatam, in præſenti exhibeo Tab. IV. *Fig.* 2. Per medium annulum metallicum *ab* (*Fig.* 1.) pice repletum, cylindrulus metallicus *cd* infixus eſt. Annulum diameter unum pollicem *Pariſinum* et quatuor lineas æquat. Cylindrulus utrinque ad diſtantiam unius pollicis prominet. Diameter annuli minor eſſe haud debet, ne cum cylindrulo communicata electricitas, ſi ad annulum metallicum pertingat, imminuatur. Ferrumine adjuncta eſt annulo *ab* furca metallica, cujus ſtylus cochleatus inditus eſt cylindro ligneo *ef*, (*Fig.* 1.) cujus extremitas inferior *f* ita formata eſt, ut per aſſeris alicujus fiſſuram pervadens huc illuc moveri, et ope cochleæ firmari queat. Ejuſmodi cylindrum metallicum pici, qua annulus repletus eſt, infixum cum ſtylo cochleato, compendii cauſa, ſimpliciter *cylindrum electricum* nomino (*Fig.* 1.). Ex quatuor hujuſmodi cylindris electricis pyrorganum electricum, *Fig.* 2. expreſſum, conſtat. Cylindri ita ponuntur, ut inter ſe habeant intervallum ad gignendas ſcintillas electricas ſufficiens. Eas vero excitaturus pyrorganum juxta aliquod metallum *ab* ex filis ſericis ſuſpenſum firmatumque ita colloco, ut inter metallum et cylindrum *c*, intervallum neceſſarium interſit. Ultimo cylindro *fg* in *g* alligo filum metallicum *h* in vas metallicum aqua plenum *i* pertingens. His ita ordinatis, ſimulac

metallum

metallum oblongum *ab* electricitate donatur, in quatuor intervallis ſcintillæ electricæ emicant tanto clarius ſplendentes, quanto majorem electricitatem communicant rotati frictique globi vitrei.

Ex quo libellus nominatus in lucem prodiit, duo ejuſmodi pyrorgana electrica conſtruxi, quorum alterum formam rotæ alatæ habet, alterum ſcintillis emicantibus ſeptemtriones repræſentant.

Rotæ alatæ conſtructio, quam TAB. IV. *Fig.* 4. delineari curavi, eſt ſequens. Orbi ligneo cavato ſeu annulo ligneo *dddd* ſex alæ ligneæ *cd* infixæ ſunt. Totius annuli cum foramine diameter 13 digitos, et foraminis rotundi diameter 6 digitos complectitur. Alæ *cd*, quarum longitudo decem digitos comprehendit, fiſſuras habent, in quibus tres cylindri electrici hinc illinc moveri et firmari poſſunt. Prope alas infixas in annulo *dddd* foramina angulata formata ſunt, in quibus ſingulis itidem cylindrus electricus ſtatuitur. Itaque in rota alata ſex ordines apparent, qui ſinguli quatuor cylindris electricis conſtant, obſervatis inter ipſos intervallis, quæ ad excitandas ſcintillas electricas proſunt. In cujuſvis alæ extremitate *c* firmatur inſtrumentum metallicum *gf*, conſtans tribus partibus, quarum extremæ ad angulum rectum mediæ *g* junctæ ſunt.

Ut vero inſtrumenta iſta metallica rite applicari queant, in latere ad finem *c* alæ poſtico (anticum enim nomino, in quo cylindri electrici ſcintillant) fibula metallica affixa inter ſe et latus alæ poſticum aliquod intervallum relinquit. Media fibula inſtructa eſt cochlea verſatili *g*. Dicto intervallo pars brevior inſtrumenti metallici inditur. Pars altera extrema *h* cylindro quarto propinqua eſt; tantum vero ab eodem

diſtat, quantum debet, ſi inter iſtam inſtrumenti partem et cylindrum ſcintilla electrica excitanda eſt. Hinc pars altera brevior ſub fibula pro lubitu moveri et firmari poteſt. Fibulis applicatum eſt et in partibus poſticis alarum circumductum filum metallicum *ik*, cui, quando ſcintillæ electricæ ſuſcitandæ ſunt, in loco *x* aliud metallum *y* adjungitur in vas metallicum *s* aqua plenum pertingens. Electricitas, ſimulac cum cylindro primo communicatur, ad omnes cylindros tranſit.

Hinc neceſſe eſt, ut, ſi ſcintillæ exoriri debeant, inſtrumenta metallica deſcripta *gf* ſemper libera et vacua maneant electricitate. Quod fit metallo *y*, quod filo metallico *ik* adjunctum eſt, electricitatem in aquam propagante. Inter duo enim corpora æquali prædita electricitate nulla exiſtit ſcintilla. Cum primo autem cylindro electricitas communicatur a malleo metallico *a* affixo axi metallico *bc*, qui in foraminibus rotundis duarum columnarum *de* lignearum verſari poteſt præditus manubrio *f*. Hujus mallei apparatum *Fig.* 5. oſtendit.

Columnæ *de Fig.* 5. ligno *mn* inſiſtunt, quod ſtylum *g* habet infixum operculo, quod vas vitreum *h* pice cum eodem copulatum tegit. Hoc vaſe vitreo ideo opus eſt, ut electricitas axi metallico *bc* ope alicujus fili metallici appenſi data conſervetur.

Vaſis vitrei *h* fundus ope picis junctus eſt cum ligno, cui ſtylus adjunctus eſt, qui foramini longiori columnæ *ik* ita inſeri poteſt, ut cochleæ *l* ope firmari queat, poſtquam vas vitreum debitam altitudinem nactum eſt. Hæc vero debet eſſe altitudo, ut axis *bc* in foramine annuli lignei *dddd* medium teneat. Cylindri autem electrici in annulo et alis *cd*, ita

ita collocandi sunt, ut malleus a quocunque cylindro electrico primo, ad quem accedit, et cylindri a se invicem sufficientem scintillis electricis excitandis distantiam habeant.

In basi quadrata *mnop*, *Fig.* 4. in qua columna *ik* insistit, fissura est, in qua, quando malleus *a* satis per foramen annuli *dddd* prominet, columna *ik* cochlea stabilitur. Annulo *dddd* furca metallica applicata est infixa columnæ ligneæ *qr*, quæ similiter in fissura basis quadratæ *mnop* firma redditur. Axis metallicus *bc* per medium annuli foramen protensus a basi quadrata per altitudinem 21 digitorum abest. Quando igitur, axe *bc* electricitatem nacto, malleus alicui cylindro electrico primo appropinquat, quinque scintillæ secundum seriem rectam uno fere tempore exoriuntur.

Verso itaque sensim axe *bc*, ordines isti scintillarum per circulum in conspectum prodeunt. Scintillæ adeo claræ sunt, ut in luce diurna ad centum pedes videantur. Homo qui axem manubrio versat, ne electricitatem dissipet atque perdat, stare debet in materia electricitatem non propagante.

Electricos Septemtriones, Tab IV. *Fig.* 3. exhibet. In tabula *abcd*, quæ in columnæ *ef* fissura elevari et demitti potest, novem cylindri electrici ita collocari possunt, ut septem scintillæ eo ordine simul appareant, quo ipsi Septemtriones in cœlo nocte serena micant. Cylindro metallico primo eo tempore, quo existere debent scintillationes, adjungitur filum metallicum *h*, cui electricitas traditur. In tertii et quarti extremitatibus filum inflexum metallicum *i* annectitur, ut electricitas ad quintum et reliquos cylindros permeare valeat. Cylindro nono filum metallicum *k* applicatur in aquam in vase metallico *l* productum, quo

electricitas omnibus cylindris tributa in materiam, quæ eandem haud conservat, quantum opus est, propagetur.

Hæc quanquam nonnisi ad delectandos oculos spectare videantur, attamen vobis, Illustrissime Præses, Viri Illustrissimi, qui in ejusmodi delectamentis electricis vim naturæ mirabilem latere perspicitis, haud ingrata fore confido. Qua spe fretus, vestro omnium favori, quem impense veneror, me submisse commendo.

Scribebam *Lipsiæ*,
die 31 *Martii*,
1747.

XVII. *Some Observations upon* Gems *or* Precious Stones; *more particularly such as the Ancients used to engrave upon, by* Robert Dingley, *Esq*;.

Read May 7. 1747.

GEMS or precious Stones, of all Species, are sometimes found of regular Shapes, and with a natural Polish; and sometimes of irregular Shapes, and with a rough Coat. The first Sort may be consider'd as of the Pebble-Kind; and they are said to be found near the Beds of Rivers, after great Rains: The others are found in Mines, and in the Clefts of Rocks.

The Gems of the first Sort were what the Ancients most usually engraved upon: These are commonly called *Intaglio's*; and they are mostly of a long oval Figure, inclining to a Point at each End, convex as well on the engraved Face, as on the others, with a Ridge

Ridge running from End to End on the under Side, which is hereby, as it were, divided into two Faces; both which are alſo, tho' not ſo diſtinctly, parted from the upper Face, by another Ridge running quite round the Oval.

The Stone moſt commonly found engraved is the *Beryl*; that moſt frequently found next is the *Plaſm* or prime *Emerald*; and then the *Hyacinth* or *Jacinth*. The *Chryſolite* is ſometimes, but rarely, found engraved; as are alſo, but that very ſeldom, the *Cryſtal*, or *Oriental Pebble*, the *Garnet*, and the *Amethyſt*.

Of the *Beryl* there are three Species; the Red, inclining to Orange-colour, tranſparent and lively; the Yellow, of an Ochre-colour; and the White, commonly called the *Chalcedon*, of the Colour of ſheer Milk. Theſe two laſt have leſs Life than the firſt.

The *Plaſm* or prime *Emerald* is green, nearly of the Colour of ſtagnated Water; ſometimes tolerably clear, but, for the moſt part, full of black and white Specks, and rather opaque.

The *Jacinth* is of a deep tawny Red, like very old *Port* Wine, but lively and tranſparent.

The *Chryſolite* is of a light-green Graſs-colour, and is ſuppoſed to have been the Beryl of the Ancients, tranſparent, but not lively

The *Cryſtal* or *Oriental Pebble* is harder and more lively than the common Rock *Cryſtal*; is of a ſilveriſh Hue, and but very little inferior to the white *Sapphire*.

The *Garnet* is of the ſame Colour as the *Jacinth*, but more inclining to the Purple, and not ſo lively.

The

The *Amethyst* is of a deep Purple, transparent and lively.

There were some other Species of Stones engraved upon by the *Romans*; but rarely before the latter Times of the Empire, when the Art itself was greatly upon the Decline.

All the before-mention'd Sorts of Stones are said to have been of the Produce of *Egypt*, or of the *East Indies*; and to have been brought from the Borders of the *Nile*, or of the *Ganges*.

Here follows a general Table of what are usually called Precious Stones.

The *Beryl*, is red, yellow, or white.
The *Plasm*, is green.
The *Jacinth*, of a deep tawny red.
The *Chrysolite*, of a light grass green.
The *Crystal*, or *Oriental Pebble*, of a silverish white.
The *Garnet*, of a deep red Claret-colour.
The *Amethyst*, purple.
The *Diamond*, white.
The *Ruby*, red or crimson-colour'd.
The *Emerald*, of a deep Green.
The *Aqua marina*, of a bluish Sea-green, like Sea-Water.
The *Topaz*, of a ripe Citron yellow.
The *Sapphire*, of a deep Sky-blue, or of a silver white.
The *Cornelian*, red or white.
The *Opal*, white and changeable.
The *Vermilion-Stone*, is more tawny than the *Jacinth*.

All

All these Stones are more or less transparent: The following are all opaque:

The *Cat's-Eye*, brown.

The red *Jasper*, called also thick *Cornelian*, is of the Colour of red Ochre.

The *Jet*, black.

Agates, are of various Sorts.

The *Blood-Stone*, is green, veined or spotted with red and white.

The *Onyx*, consists of different parallel *Strata*, mostly white and black.

The *Sardonyx*, of several Shades of brown and white.

The *Agat-Onyx*, of two or more *Strata* of white, either opaque or transparent.

Alabaster, different *Strata* of White and Yellow, like the *Agate-Onyx*, but all opaque.

The *Toad's-Eye*, black.

The *Turquoise*, of a yellowish Blue inclining to green.

Lapis-Lazuli, is of a fine deep Blue.

Of most of the Species before-mention'd there are some of an inferior Class and Beauty. These are commonly called by Jewellers Occidental Stones: They are mostly the Produce of *Europe*, and found in Mines or Stone-Quarries; and are so named, in Opposition to those of a higher Class, which are always accounted Oriental, and supposed to be only produced in the more Eastern Parts of our Continent.

The

The *Onyx*, *Sardonyx*, *Agate-Onyx*, *Alabaſter* of two Colours or *Strata*, as alſo certain Shells of different Coats, were frequently engraved by the Ancients in *Relief*; and theſe Sorts of Engravings are commonly called *Cameo's*. They alſo ſometimes ingrafted a Head, or ſome other Figure in *Relief* of Gold, upon a *Blood Stone*.

Beſides which there are ſome Antiques, moſtly *Cornelians*, that are cover'd with a *Stratum* of White. This *Stratum* has by ſome been look'd upon as natural; but it was really a ſort of Coat of Enamel that was laid on. This was uſed only in the Times of the lower Empire.

The Stones eſteemed the beſt for engraving upon were the *Onyx* and *Sardonyx*; and next to them the *Beryl* and the *Jacinth*.

The Ancients engraved moſt of their Stones, except the *Onyx* and the *Sardonyx*, juſt as they were found; their natural Poliſh excelling all that can be done by Art; but the Beauty of the ſeveral Species of *Onyx*'s could only be diſcover'd by cutting.

The Merit both of *Intaglio's* and *Cameo's* depends on their Erudition, on the Goodneſs of the Workmanſhip, and on the Beauty of their Poliſh.

The antique Gems of *Greek* Work are the moſt eſteemed; and next to them the *Roman* ones, in the Times of the higher Empire.

XVIII.

XVIII. Quadrantis *Astronomici* Muralis *Idea nova et peculiaris, multis incommodis quibus hucusque usitati laborant liberati, Autore* Christiano Ludovic. Gersten, *R.S.L.S.*

Read May 7. 1747.

INSIGNEM utilitatem arcuum parietibus in meridiani plano firmiter infixorum nemo nisi hospes in astronomiæ studio ignorat. Factum inde, ut nullum hodie facile detur observatorium supellectile sua instructum quod his careat; repertum tamen nullum dari murum tam solidum atque inconcussum, aut tam validam crassamque ferri et metallorum compagem, ut omnibus hisce organon hoc invariatum atque prorsus immobile respectu axis terrestris effici queat. Cogitavi itaque de novo artificio; arcum nempe muralem construendum propono telescopio et micrometro instructum, cujus requisita sint sequentia.

I. Ut quovis momento videri possit utrum planum instrumenti verticaliter positum sit: et,

II. Utrum perpendiculum per centrum quadrantis et principium divisionis limbi exacte transcat.

III. Ut aberratio plani quadrantis a linea verticali corrigi possit, manente positione principii divisionis in limbo respectu perpendiculi: rursusque,

IV. Aberratio principii divisionis in limbo a perpendiculo corrigi possit, absque mutatione notabili positionis plani quadrantis respectu lineæ verticalis.

V. Ut pariter deviatio plani quadrantis a plano meridiei emendari possit, manente situ perpendiculari

tum plani quadrantis tum principii divisionis in limbo.

VI. Ut penitus expers sit variationis quam extensio metallorum per calorem et frigus producere potest. Denique,

VII. Ut facile possit rectificari uti dicunt, hoc est, Ut facile videri possit utrum linea ab objecto per intersectionem filorum in tubo ad oculum transiens sit exacte parallela lineæ per centrum quadrantis et divisionem quam regula monstrat transeuntis, atque ubi opus est facile in rectum situm collocari. Negotium alias admodum operosum et difficile.

Ut omnia ista obtineantur, fiat,

1. Fulcrum ferreum *aaa*, *ccb*, (TAB. V. *Fig.* 1. & 2.), cujus pars quæ muro applicatur *Fig.* 1. altera *Fig.* 2. delineata. Consistit ex norma ferrea *aaa*, et regula transversa *cc*, ad ipsam normam claviculis mallei ope firmiter consolidata. In *b*, normæ brachium horizontale est inflexum, ut in tergo promineat, habens insuper foramen rotundum horizontale, cujus usus mox inferius indicabitur, aliudque verticale minus cochleæ striis excavatum, ut cochlea mas, *m*, huic immitti possit.

2. In tergo fulcri ad brachium verticale normæ claviculis consolidatæ sunt ansæ duæ *ehk* et *dg*. Superior *ehk* desinit infra in cylindrum *h*, et cochleam marem *k*. Inferior *dg*, ad *g*, in superficie inferiori conice excavata; ut axis cylindri *h*, et apex coni *g*, sit in eadem linea, eaque plano normæ anteriori *aa*, *Fig.* 2. parallela postulo, nec tamen ut summa præcisio in his adhibeatur.

3.

3. In ipſum murum plano meridiei utcunque parallelum immittantur, et idonea compage firmentur tres mutuli ferrei (*Fig.* 4.) *ah*, *gb*, *ce*; quorum duo, *ha* et *gb*, propemodum æquales, et in eadem linea perpendiculari exiſtunt. Superior *ha* habet foramen cylindriacum *h*, tantæ capacitatis, ut cylindrus fulcri *h* (*Fig.* 2.) eidem immitti poſſit. Inferior mutulus *bg* habet loco foraminis apicem conicum *g*, qui in cavum anſæ *g* (*Fig.* 1.) intrare poſſit. Diſtantia inter hos mutulos tanta, ut immiſſo in foramen *h* (*Fig.* 4.) cylindro *h* (*Fig.* 1 et 2.) et in cavum *g* (*Fig.* 1.) apice *g* (*Fig.* 4.) omne fulcri pondus apex *g* ſuſtineat, libereque fulcrum horizontaliter volvi poſſit. Anſæ igitur ſuperioris pars *c* (*Fig.* 2.) ipſum mutulum non premere, ſed aliquantiſper ab eo diſtare debet. Ne tamen ex cavo *g* (*Fig.* 1.) apex *g* (*Fig.* 4.) excidere aut vacillare poſſit, addatur cochlea fœmina ad marem *k*, cujus ope premendo inferiorem partem mutuli *ah*, fulcrum verticaliter ad apicem *g*, tantum deprimitur quantum ſat eſt.

4. Ut cylindrus *h* (*Fig.* 2.) in foramine *h* (*Fig.* 4) ab omni vacillatione penitus ſit ſecurus, addatur alia cochlea minor horizontalis *f*, vel duo ex oppoſitis plagis, cylindrum in foramine tangentes. Ut axis foraminis *h* et apex *g* (*Fig.* 4) ſit in eadem perpendiculari, hujus effectio nihil habet difficultatis in praxi, quoniam in ipſa muri ſtructura hi mutuli ſic diſponi poſſunt. Primum inferior *gb*, deinde ſuperior *ha*, ope perpendiculi cuſpidati, cujus filum per axin diſci ænei cavitatem foraminis *h* exacte replentis tranſire debet.

5. Tertius mutulus *ce* (*Fig.* 4.) conſiſtit in craſſa cochlea mari, longe ſatis ex muro prominente. Fo-

tamen *b* (*Fig.* 2.) fulcri tantæ capacitatis, ut hanc cochleam transmittat, ipsius cochleæ superior pars ademta, ut habeat planum horizontale, cui cochlea *m* (*Fig.* 2.) inniti possit. Immisso itaque cylindro *h* (*Fig.* 2.) in foramen *h* (*Fig.* 4.) et cavo *g* ansæ *d*, (*Fig.* 2.) applicato ad conum *g* (*Fig.* 4.) additaquè cochlea fœmina ad *k* (*Fig.* 2.) eademque applicata ad partem inferiorem mutuli *ab* (*Fig.* 4.) ad cochleam mutuli *ce* applicetur cochlea fœmina quædam plana, orbicularis et in margine dentata, ut eo melius clavi quodam expresse ad id confecto circumagi possit, adigatusque usque ad partem mutuli *c* propemodum. Deinde horizontaliter volvendo ipsum fulcrum, cochlea *e* (*Fig.* 4.) foramini brachii horizontalis fulcri *b* (*Fig.* 2.) immittatur, circumvolvetur cochlea *m*, eo usque donec planum cochleæ crassæ mutuli *e* attingat, et ipse mutulus adeo ponderis fulcri partem aliquam sustineat. Deinde addatur adhuc alia cochlea fœmina plana et dentata ad eandem marem *e* (*Fig.* 4.); agatur usque ad planum brachii horizontalis normæ, id quod in nullum alium finem flexum, nisi ut hanc cochleam fœminam sic recipiat, ne suspensioni quadrantis in fulcro obstet, atque ut eo major concedatur longitudo cochleæ crassæ mutuli *e*. Sic fulcri pars *b* (*Fig.* 2.) nititur hoc modo verticaliter in cochleam mutuli *ec* (*Fig.* 4.) et in positione azimuthali servatur per duas descriptas cochleas fœminas dentatas mutuli *be*. Quodsi igitur aliquid in positione plani muralis aberratum, istud semper quo tempore libuerit, mediantibus hisce cochleis dentatis, corrigi potest. Me non monente, benignus lector intelliget foramen brachii horizontalis *b* (*Fig.* 2.) satis amplum et ovalis figuræ esse debere, ne angustia motui azimuthali obstet.

6. Fulcri

6. Fulcri pars anterior (*Fig.* 2.) tres rursus habet mutulos *n*, *o*, et *p*. Primus *n* est ad formam cubi aut parallelipipedi, nisi quod superficies ejus superior sit semicylindriace excavata. Secundus *o* unci formam habeat (*Fig.* 3.) specialiter depictam. Tertius *p* nihil aliud est nisi cochlea mas prominens. Cuncti ad normam quoad fieri potest optime consolidati.

7. Quadrans ipse ut ex solido sit metallo postulo. Facies ejus anterior exhibetur TAB. VI. *Fig.* 1. Crassities sit congrua, limbusque quadrantem idonea quantitate excedat. Ad planum limbi rectificandum adsit regula composita *kk*, ex duabus, quarum una *rr* perpendiculariter insistit ad planum alterius *kk*, sic ut in neutram partem facile flecti possit. Regulæ *rr* acies rectitudinem habeat perfectam, exhibeatque lineam rectam quæ sit in plano superficiei limbi anterioris. In tergo per cochleas hæc regula quadranti firmatur, sicuti ex *Fig.* 2. apparet. Quodsi enim hæc regula plano limbi in *m* et *n* bene coincidit firmata ad centrum alia regula rectilineari examinatoria, facile patebit utrum limbus et acies *rr* regulæ in eodem sint plano, per consequens limbi planum recte se habeat. Nam ex punctis *m* et *n*, nonnisi unica recta duci potest, quæ per hypothesin in acie regulæ *rr* revera existit. Per lineam rectam vero *mn*, et punctum *a*, nonnisi unicum planum duci potest. Quodsi igitur regula examinatoria ad *a* affixa ubique aciem regulæ *rr* et limbum quadrantis exacte tangit, planum limbi in plano trianguli *anm* necessario esse debet. Post examen et correctionem hæc regula *rr* superflua est, adeoque, si libet, adimi potest.

8. In tergo quadrantis (*Fig.* 2.) duo ſint ſuſtentacula orichalcea, *ab* et *ef*, ad ipſius quadrantis ſuperficiem per cochleas probe firmata. Suſtentaculum *ab* in *b* habeat foramen ovale; in lateribus ſint duo cochleæ *c* et *d* cuſpidatæ. Foraminis parietes ſuperne ſint convexæ, ut immiſſo mutulo *o* (TAB. V. *Fig.* 2.) in hoc foramen, pars cava *a* mutuli (*Fig.* 3.) repleatur, cuſpideſque cochlearum horizontalium *c* et *d* (TAB. VI. *Fig.* 2.) partem mutuli aut unci convexam inferiorem (TAB. V. *Fig.* 3.) ex duabus oppoſitis partibus ſtringant, et ſic omnis vacillatio excludatur.

9. Alterum ſuſtentaculum *ef*, in *e* cochleis ad planum quadrantis affixum, ad *f* habet foramen rectangulare, quod ingrediuntur cochleæ mares verticales *h* et *i*, quarum uſus eſt ſequens. Immiſſo mutulo ſive unco *o* (TAB. V. *Fig.* 2.) in foramen ſuſtentaculi *ab* (TAB. VI. *Fig.* 2.), mutulus *n* (TAB. V. *Fig.* 2.) quoque immittitur in foramen rectangulare ſuſtentaculi *ef*, et cochleæ *h* apex, qui hemiſphærice convexus eſſe debet, in cavo mutuli *n* (TAB. V. *Fig.* 2.) ſubſiſtat, et ſic per motum cochleæ *h* ſitus quadrantis elevando et deprimendo parumper eſt variabilis. Foramen *f* in hunc finem ſatis amplum eſſe debet; quando tamen ſemel ſitus conveniens per cochleam ſuperiorem determinatus, tunc vacillatio quadrantis non proficua, ideo per cochleam inferiorem *i* firmiter ad mutulum conſtringitur.

10. Duobus hiſce ſuſtentaculis quadrans in ſitu debito quidem teneri, ac, ubi opus eſt, corrigi poteſt, reſpectu principii, diviſionum in limbo, ut iſtud nimirum perpendiculo congruat per centrum quadrantis ducto. Sed requiritur ulterius, ut et ipſum quadrantis planum ſit perpendiculare. Ut id efficiatur, adſint

adsint duo cochleæ fœminæ planæ orbiculares, cochleam marem *p* (Tab. V. *Fig.* 2.) complectentes; harum prima eaque dentata, mari *p*, applicetur antequam quadrans mutulis fulcri sit applicatus. Postea vero, ubi quadrans sustentaculis *cd* et *fe* (Tab. VI. *Fig.* 2.) mutulis *o* et *a* (Tab. V. *Fig.* 2.) pendet, atque cochlea mas *p* in foramine *c* (Tab. VI. *Fig.* 1 et 2.) existit (quod sat amplum et ovalis figuræ esse debet, ut concessa sit variatio quædam situs, per cochleam ansæ superiorem *h* (Tab. VI. *Fig.* 2.) efficiendam); tunc tergum quadrantis sustinetur cochlea fœmina orbiculari dentata, ad marem *p* (Tab. V. *Fig.* 2.) applicata, paulo superius descripta, facies vero per alteram cochleam fœminam orbicularem, clavi quadam, foraminulis nonnullis in hunc finem terebratis intrusa, circumagendam. Planum itaque harum cochlearum fæminarum, sat amplum esse debet ne in ipsum foramen quadrantis ingrediantur. Et sic his duabus cochleis non modo valide constringitur quadrans ab utraque parte, sed etiam, ubi opus est, retro vel antrorsum promovetur, quoniam suspensio in mutulis *o* et *n* (Tab. V. *Fig.* 2.) huic motui non obstat. Quoniam tamen nimia longitudo cochleæ *p* obest, consultum erit cochleam anteriorem fœminam tali figura construere, quam sectio (Tab. VII. *Fig.* 2) exhibet, ubi margo *ab* faciem anteriorem quadrantis tangere, collum *cd* vero ipsum foramen *c* (Tab. II. *Fig.* 1.) ingredi debet.

11. Centrum quadrantis (Tab. VI. *Fig.* 1.) *a* cylindrice excavatum est, ut clavum regulæ admittat. in tergo quadrantis, mediantibus cochleis affixa est lamina *mn* (*Fig.* 2.) foramen habens quadratum *m*, centro respondens; sitque hoc quadratum, circulo foraminis

foraminis *a* (*Fig*. 1.) inscriptum, aut paulo minus. Lamina *mn* (*Fig*. 2.) crassitiem habeat congruentem, sitque dupliciter ad normæ modum flexa, desinatque in facie in partem *b* (*Fig*. 1.) a plano quadrantis satis distantem, ut stylum tenuem centrum clavi pungentem, filumque perpendiculi sustinentem teneat. Ipsum clavum (*Fig*. 3.) specialiter delineavi, in eoque est *a* caput, *b* cylindrus cavum centri quadrantis, et regulæ exacte complens, *c*, quadratum foramini *m* (*Fig*. 2.) immittendum, *d* cochlea mas, cui conveniens fœmina applicanda.

12. Tab. VII. *Fig*. 1. exhibet quadrantem cum regula et perpendiculi apparatu: *pr* est linea superficiei quadrantis inscripta, per centrum, si continuata fuerit, transitura, qua divisionum in limbo principium est. *hikg* est parallelipidum eo modo, sicuti figura indicat, excavatum, cochleis ad planum quadrantis affixum, quo filum *mo* perpendiculariter imminet lineæ *pr*, quod normæ accuratioris ope nullo negotio perficitur. Alterum filum *ik* situm habet ad planum quadrantis parallelum, ad tantam tamen exacte distantiam, quanta est altitudo centri in clavo *c*, sive punctum suspensionis fili supra superficiem quadrantis. Addatur tertium, secundo prorsus simile in latere opposito parallelipipedi *hgv*, cujus delineationem scenographiæ leges vetant.

13. Ipsum filum stylo subtili centrum clavi *c* pungente suspensum, ex pilis capitis humani constet, ponduscul um *f* semiunciæ facile sustinens, et in cavo parallelipipedi *hikg* libere oscillet. Fila minora parallelipipedi *mo*, *ik*, &c. etiam ex iisdem pilis constent, habeantque crassitiem æqualem. Usus eorum hic est; ut facile cognoscatur positio quadrantis

tis reſpectu perpendiculi *cf*. Nam per filum *mo*, ad lineam *pr* collimando, exactum datur oculorum judicium, utrum *pr*, principium diviſionum in limbo, cum perpendiculo *cf* conveniat. Rurſuſque per filum *ik* ad alterum oppoſitum collimando, videri poteſt utrum planum quadrantis ſit perpendiculo parallelum. Quod ſi loco parallelipipedi duo ponticuli, hæc tria fila ſuſtinentes ſubſtituantur, idem finis brevius adhuc obtinebitur, cumque ſpatium oſcillationis fili *cf* exiguum ſufficiat, diſpoſitio horum trium filorum talis eſſe poteſt, ut per vitra convexa collimatio fieri poſſit; quod commodum accidet, quibus obtuſior oculorum acies. Cochleæ fœminæ *b* margo orbicularis, adeoque et ipſa cochlea mas fulcri *p* (Tab. V. *Fig.* 2.) quam complectitur, ſic ultra ſuperficiem quadrantis promineat, ne oſcillationem fili *cf* impediat, aut, ſi mavis, iſtarum cochlearum et foraminis in quadrante locus extra oſcillationis fili ſpatium aſſignandus. Quodſi obſervatorii ſtructura patiatur, ut ab horizonte ad *Zenith* ſtellarum detur aſpectus, tunc regula ad lineam *pr* acceſſum debet habere liberum, adeoque parallelipipedum *hikg* paulo inferius in appendicem collocandum, aut ſuperius paulo in brachium verticale, ipſaque appendix inciſionem convenientem habere debet.

14. Progredior nunc ad ipſam regulam, quæ quidem Tab. VII. *Fig.* 1. quoad maximam partem ſcenographice delineata, atque literis *nnnn* deſignata extat, inſuper tamen habita juſta magnitudine et proportione partium, quam in omnibus hiſce figuris negligendum duxi, quoniam ſic multo diſtinctius mentem meam figuris ſpectabilioribus explicare potui. Hæc regulæ ſtructura, quoniam ea plane peculiaris, ſpe-

cialius nunc mihi explicanda. *nnnn* est planum regulæ circa clavum *c* volubilis. Contra incurvationis periculum munita est alia regula *ddd*, perpendiculariter ad planum *nnn* afferruminanda, aut alio idoneo modo consolidanda. Divisiones limbi indicantur in apertura, aut fenestra regulæ *xx*; ibique aut per filum extensum, aut per aciem vel marginem interiorem aperturæ, aut per divisionem quam vocant Nonianam, prout libuerit. *qq* est telescopium. Quod si nunc ponamus regulam exacte gradum *o* vel 1 altitudinis in limbo monstrare, nihil erit quod in organo desiderares, nisi ut linea ab objecto per decussationem filorum telescopii ad oculum pergens, sit exacte parallela lineæ per centrum quadrantis et gradum *o* vel 1, altitudinis in limbo transeuntis. Sed cum prima vice tubus talem in modum regulæ consolidari nequeat, ut huic conditioni satisfiat, postulo eum sic regulæ connecti, ut in situ, quo Tab. VII. *Fig.* 1. depictus est, motum aliqualem habeat non solum in altitudinem, sed etiam in partes azimuthi. Motus in altitudinem perficitur per cochleas *uu*, motus azimuthalis vero per cochleas *ww*, quæ omnia tamen specialius et distinctius Tab. VIII. delineata extant.

15. Motus in altitudinem efficitur sequentem in modum. Tubo loco convenienti afferruminatur pes aut ponticulus Tab. VIII. *Fig.* 6. depictus; basis *bck* est plana et rectangularis, ad *b* et *ck* crassities metalli minor. Pars *i* perpendiculariter ad basin afferruminata, huic connectitur altera pars volubilis *namh*, mediante stylo *h*, juncturas utriusque partis transeundo. In *a* pars volubilis excavata, tali curvedine ut tubum recipiat stanno in hunc canalem afferruminandum. Basin hujus pedis *b'c k* ad planum regulæ deprimunt, duo

frusta

frusta orichalcea, quorum figuram *Fig.* 7. sistit, et quos *depressores pedis* voco. Habent autem hi depressores duo foramina *k* et *m*, ad recipiendas cochleas, et duos apices *h* et *i* foraminulis in plano regulæ in hunc finem terebratis intrudendos. Melius nunc omnia ex ichnographica delineatione horizontali *Fig.* 2. intelligentur, ubi *aa* tubi pars, *bc ee* pes, *ee* basis pedis, *bc* pars volubilis tubo afferruminata, *ff* duo depressores pedis, *gg* cochleæ mares, matrices in regula excavatas ingredientes, et hoc modo pedem valide prout libuerit ad planum regula deprimentes. *Fig.* 5. exhibet delineationem verticalem, in eaque *aa* tubi pars, *ee*, *ff*, cochleæ depressorum; ipse pes sub tubo latet: *g*, *h*, sunt duæ cochleæ mares, quarum apices planæ ab utraque parte pedem stringunt immotumque servant, et ubi situs horizontalis tubi elevando vel deprimendo mutandus, id facile per revolutiones harum cochlearum perficitur: sed tunc cochleæ depressorum pedis *ff*, *ee*, non nimis fortiter pedem ad planum regulæ deprimere debent.

16. Sequitur nunc motus ad partes azimuthi. Quod *Fig.* 9. delineatum, vocemus *tabulam plicatam.* Consistit ex plano rectangulari orichalceo *kfgh*, cui ad angulos rectos insistat aliud planum *abcf*, cujus latera tamen *af* et *bc* sint in curvedine circuli, ex centro foraminis *q* ducti, margines *af* et *dbcg* sunt ad angulos rectos excavati, prorsus eodem modo sicuti basis pedis (*Fig.* 6.). Cui respondere debent duo depressores similes ei quem *Fig.* 7. sistit, nisi quod curvedinem ipsi *bc* convenientem habere debeant. Planum *kfgh* aperturas habet rectangulares *mrn* et *osp* pro recipiendis appendicibus tubi quas mox describam. In *Fig.* 8. *aa* est tubi pars, *ee* ta-

bulæ plicatæ pars verticalis, mediante clavo *b*, ad planum regulæ firmata, atque circa clavum volubilis, *cc* et *dd*, depressores tabulæ, atque earum cochleæ. *hh* est sectio partis tabulæ horizontalis, quæ *Fig.* 9. literis *kfgh* indicatur. *gg* sunt appendices peculiares, tubo afferruminatæ; desinunt in cochleas mares, et pars earum foraminibus rectangularibus *mn* et *op* (*Fig.* 9.) respondet, sic ut secundum longitudinem *mr* aut *os* possint intervallo quodam apto hinc illincque promoveri, et tamen mediantibus cochleis fœminis *kk*, interpositis antea orbiculis *ii*, quando libuerit ad ipsum planum tabulæ valide firmari. Quodsi igitur Tab. VII. *Fig.* 1. per cochleas *uu* contra pedis parietes applicatas telescopium attollitur aut deprimitur, tabulæ complicatæ clavus istud permittit, relaxatis paulisper cochleis depressorum *xx*. Rursus quando ad motum azimuthalem peragendum, appendices *ww*, relaxatis cochleis fœminis hinc illincque, per consequens ipse tubus promovetur, pedis junctura id quoque permittit. Quodsi appendices *g*, *g*, (*Fig.* 8. Tab. VIII.) transverse valido satis metalli frusto connectuntur, hic motus azimuthalis unius cochleæ artificio et lentus et securus effici potest.

17. Præterea habet regula appendicem peculiarem tergum limbi radentem. Constat Tab. VIII. *Fig.* 1. et 2. ex parte normaliter inflexa *Am*, plano regulæ validissime consolidata et altera volubili *ko*, stylo *m*, juncturis suis cum immobili *Am* connexa, in parte volubili orbiculus *i Fig.* 2. circa cylindrum *q* in cochleam desinentem versatilis tergum limbi radit, atque adversus istud constringitur claustro *ns* cochleis *ssss* (*Fig.* 1.) firmato. Mota regula ad quamlibet deinde divisionem firmatur ope cochleæ *v*, cui objicitur lamina

tenuis

tenuis *ut Fig.* 2. ne apex metallum limbi excavet, contactum immediatum impediens.

18. In vitris telescopii nullam requiro quam vocant centrationem, hoc est, ut maxima crassities sit in medio vitri, negotium alias tædiosum et operosum. Hoc tantum postulo, ut vitra præprimis objectivum constantem habeant situm in tubo, in quem, si ad abstergendum forte amoveantur, facile rursus restitui possint. Angli artifices, passim vitra ocularia telescopiorum præprimis reflectentium, cylindris cochleæ helicibus incisis firmiter infigunt, quo deinde aptis matricibus tubo immittunt. Quod si igitur helices cochleæ sunt bonæ, et præterea signum quoddam efficitur in cylindri margine, alii in cavo tubi facto respondens, necessario vitrum eundem situm servabit vel centies amotum, modo sic tubo rursus inseritur, ut signum signo respondeat.

19. Micrometrum, Tab. VIII. *Fig.* 4. habet collum, cujus margo in octo æquales partes lineis ad centrum convergentibus, scrupulose, aut si plura fila verticalia requiruntur in alias quascunque partes divisus. Ad has divisiones fila sive serica sive metallica facile applicantur, firmanturque in collo aut ceræ ope, aut subtilioribus claviculis, ex buxi ligno aut metallo confectis. *a* est prominentia quadrangularis, habens in parte opposita similem aliam, sed aut majorem aut minorem: ambarum crassities æqualis esto aut paulo minor crassitiei tubi cui immittitur. Ipse tubus *Fig.* 3. delineatus, in *d* habeat crenam rectangularem, prominentiam annuli *a* (*Fig.* 4.) absque vacillatione recipientem. Ipse annulus micrometri bene cavo tubi conveniat, intrususque tubo parietes interiores ejus bene contingat. *b* est vitrum oculare, a filis

micrometri

micrometri debito modo diſtans. *f* foramen oculare, operculo cochleato *b* inciſum. Remoto hoc operculo aliud ſubſtitui debet vitris fuligine obductis munitum. Ut micrometri filum horizontale ſemper ſit in eodem ſitu, aut ſi diſturbatum, ad debitum ſitum rurſus redigi queat, tubus major teleſcopii *aa* (*Fig*. 5.) deſinat in cuſpidem *i*. Inſerto deinde tubo oculari *b*, et in debitum ſitum redacto, linea in ſuperficie externa tubi *b*, ad ductum marginis horizontalis cuſpidis *i* ducatur, et cochleis minutioribus poſtea hi duo tubi *a* et *b* ad ſe invicem firmentur. Quodſi micrometrum poſtulatur, filo mobili inſtructum, tunc ſtructura convenienter mutanda.

20. Nunc ad rectificationem regulæ progrediendum: Fiat ex ligno ſicco et ſolido aſſer, ſuperficiem habens horizontalem ejuſdem ferme longitudinis et latitudinis cum regula. In extrema ſuperficiei horizontalis parte ſit verticaliter erectus clavus æneus, in cochleam ſuperne deſinens, cavitatem foraminis centralis exacte replens. Alterum extremorum planum habeat, aut tabulam æneam parallelipipedalem prominentemque. Tab. VII. *Fig*. 3. *hh* denotat partem aſſeris, *fegk* tabulam æneam prominentem, cochleis *eeee*, probe ad ipſum aſſerem firmatam, ſic tamen ne capita cochlearum in ſuperficie tabulæ promineant. Ipſa metalli ſuperficies ſuperior ſit probe polita, congruat cum plano ſuperiori aſſeris, ſitque in eo ducta linea ſubtilis *ab*, quæ, ſi continuata fuerit, axin clavi tranſeat. Sint porro duo parallelipipeda orichalcea *cd* duabus cochleis, ad ipſam tabellam firmata, ſed in apta ab extremo *gk* diſtantia, ex ſequentibus mox colligenda. Singula infra habeant quatuor apices cylindriacos foraminulis in tabulam terebratis intruſos, ut

ſitus

ſitus eorum ſit quam firmiſſimus; duo hæc parallelipipeda ſe mutuo exacte contingant, et planum contactus ſit perpendiculare ad lineam *ab*. Quodſi igitur diviſiones limbi quadrantis monſtrantur per marginem interiorem aperturæ *xx* (TAB. VII. *Fig.* 1.), aut filum quoddam per aperturam iſtam extenſum tunc regula quadrantis primum ſic imponitur aſſeri, horizontaliter primum collocata ejus ſuperficie ſuperiori, ut clavus verticalis aſſeris per foramen centrale tranſeat, teleſcopio ſurſum ſpectante, et ſic remoto parallelipipedo *d* (*Fig.* 3.) margo aperturæ *xx*, diviſiones indicans, (*Fig.* 1.) aut filum extenſum exacte applicetur ad ſuperficiem perpendicularem parallelipipedi *c* (*Fig.* 3.); quo facto margo aperturæ aut filum extenſum erit in plano ad lineam *ab* perpendiculari. Porro firmata regula ad tabulam *fg* ope cochleæ *v* (TAB. VIII. *Fig.* 1. et 2.) teleſcopium cum ipſo aſſere dirigatur ad objectum quoddam remotum, immobile; notetur in eo punctum, quod per decuſſationem filorum obtegitur, immotuſque maneat in iſta poſitione aſſer. Poſtea invertatur regula, removeatur parallelipipedon *c*, et in priſtinum locum reſtituatur *d* (TAB. VII. *Fig.* 3.); margo aperturæ *xx* vero (*Fig.* 1.) ſive filum applicetur ad planum perpendiculare parallelipipedi *d*. Cumque hoc ſtatu minima pars regulæ aſſeri incumbat, adeoque neceſſe ſit ut reliquum cum teleſcopio præponderet, fulcrum quoddam cochleis inſtructum aſſeri combinandum. Applicata ergo ſitu inverſo regula ad idem planum perpendiculare lineæ *ab* (*Fig.* 3.) denuo ad objectum per teleſcopium collimandum, videndumque an punctum decuſſationis filorum ſit in eodem objecti puncto ac antea. Hic caſus ſi extiterit, nulla correctione opus habet regula. Nam cum

immota

immota fuerit recta *ab*, per centrum clavi regulæ transiens, idemque objecti punctum per telescopium tam in situ recto quam inverso apparuerit, necesse est ut linea ab objecto per decussationem filorum ad oculum transiens parallela sit lineæ *ab*. Sed cum hic casus vix unquam prima vice sic existere possit, sed semper fere aliud objecti punctum fila decussantia tangant, tunc aberratio potest esse aut in altitudine, aut in azimutho, aut in utrisque simul. Utroquecasu telescopii positio per cochleas *uu* et *ww* Tab. VII. *Fig.* 1. ad dimidium aberrationis angulum corrigenda, quantum sc. oculorum judicio hoc affirmari potest. Deinde regula rursus in situ recto modo, uti antea, imponenda, noviterque per telescopium objectum contemplandum, mox rursus invertenda, videndumque an idem punctum appareat. Si secus, telescopii positio denuo dimidii erroris quantitate corrigenda. Examen hoc toties repetendum, quoties differentia adhuc aliqua appareat. Re perfecta omnes cochleæ *dd*, *cc*, *kk* (Tab. VIII. *Fig.* 8.) et *ee*, *ff*, *gh* (*Fig.* 5.) firmiter adstringendæ, ut telescopium in eo statu permaneat. Cui hæc multiplex regulæ in assere inversio, quæ tamen sat brevi tempore absolvi potest, adhuc tædiosa nimis videbitur, is aut micrometrum addat cum filo mobili, aut, quod ego mallem, tabulam albam *bcde* (Tab. V. *Fig.* 6.) quæ duas fascias nigras *hl*, *gf*, habeat ad angulos rectos, situ horizontali et verticali sese decussantes, ad congruam distantiam disponat, et sic deinceps asserem cum telescopio situ recto imposito sic dirigat, ut punctum decussationis fasciarum sit in puncto decussationis filorum in tubo. Immoto assere, sed inversa tamen regula, prope tabulam adsit socius, mandata observatoris per signa data

data exequens, habeatque iste duas alias fascias nigras *km* et *no*, quas ad nutum observatoris situ ad *gf* et *hl*, parallelo (facile structura aliqua peculiari in assere sic efficiendo) et quidem *no* horizontaliter, *km* vero verticaliter moveri possit. Quodsi enim inversa regula in assere punctum decussationis cadit in tabulæ punctum, v. gr. *g*, tunc socius utrasque fascias *km* et *no* successive sic disponat, ut filum verticale tubi cadat in fasciam *he*, et horizontale tubi in fasciam tabulæ *km*. Hoc peracto, intervalla *fo* et *kh* ope circini bisecanda, atque fasciæ in tabula motu parallelo in ista bisectionis puncta collocandæ. Tunc immoto assere regula cum telescopio in situm rectum rursus restituitur, atque telescopii positio tamdiu per cochleas corrigitur, donec punctum decussationis fasciarum in tabula coincidat cum puncto decussationis filorum in tubo. Quodsi prima vice inversa regula et tubo ad tabulam collimatur, secunda vice vero in situ recto, tunc puncto decussationis in tabula notata, atque debito loco collocatis fasciis mobilibus, error in eodem situ regulæ corrigi potest. Et sic tubi situs respectu regulæ erit talis, ut vel in recta vel in inversa positione telescopii, idem objecti punctum in tubo conspiciatur, per consequens regula quadrantis erit rectificata.

21. Quod si fieri nequeat quin hæc regula paulo evadat ponderosior, pro commoda ejus ad astra directione, duobus modis et huic incommodo mederi potest. Primus est quem dudum *Flamstedius* sextanti suo adhibuit, atque in *Historia Cælesti* descripsit. Cochlea nimirum perpetua in regulæ appendice versatili margo exterior limbi quadrantis denticulis congruis incisus raditur. Sed tunc apparatum appendicis *sq* (Tab. VIII. *Fig.* 2.) non omittendum suadeo,

ut regulæ planities plano limbi bene sit contigua. Nam in sextante murali *Petropolitano*, qui a *Rowlejo* præstantissimo sui temporis artifice confectus est, hoc vitium deprehendi, ut margo aperturæ *xx* (TAB. VII. *Fig.* 1.) divisiones limbi ibi indicans, satis bene quidem limbum stringat, reliqua vero pars regulæ nimium a plano limbi distet, unde telescopium vacillat, vel validissime ad marginem adstricta cochlea. Sed totum hoc artificium nimis operosum mihi videtur, nec satis commodum in observationibus. Nam cochleæ revolutionibus limbi divisiones aut examinare, aut corrigere, aut comparare, nullo modo satis tutum mihi videtur; pondus regulæ sublevare, facilioremque tubi ad astra directionem hoc artificio efficere, id admodum nimis operosum. Mallem ego alio uti centies simpliciori. Sit TAB. V. *Fig.* 5. *abc* quadrans muralis, *ad* regula; in *m*, centro *a* quadrantis verticali statuatur axis vectis ferrei, *gmf*, sic ut brachium *mf*, et ejus revolutio, sit quamproxime in plano quadrantis; alterum *mg*, in alio parallelo, atque distanti. Longitudo *mf* sit dimidium circiter longitudinis regulæ *ad*, longitudo *mg*, vero 3 vel 4 pollicum. Angulus *g m f* punctorum nempe rotationis *m*, et suspensionum *gf*, sit rectus. Ad *f*, catenulæ vel funiculi ministerio connectatur regula, sic ut *mf* sit æqualis et parallela *ae*. Ad *g* alligetur funiculus *gh* horizontaliter ad extremum conclavis extensus; ibi trochlea *h* fultus, tandemque pondere *k* appenso ad terram propemodum demissus. Quodsi enim hoc pondus *k* regulæ sufficiens efficitur, in quocunque situ, ea vel demissa vel in altum sublata manebit. Parum obstabit, quod centrum gravitatis regulæ nostræ a puncto suspensionis distans futurum sit, quoniam

pressio

preſſio elaſtri in tergo limbi inæqualem gravitatis actionem ſatis temperabit.

22. Reſtat nunc ut oſtendam organum deſcriptum cunctis requiſitis ſuis ſatisfacere.

I. Cum fila *i*, *k*, (Tab. VII. *Fig.* 1.) et alterum ipſi ſimile in oppoſita parte parallelipipedi ſitum habeant ad planum quadrantis parallelum, eandemque diſtantiam ac punctum ſuſpenſionis *c*, facili negotio collimando patebit, utrum filum perpendiculi ſit in plano horum filorum, adeoque planum quadrantis in poſitione verticali. Quod erat unum.

II. Quoniam filum *mo* eſt in plano lineæ *pr*, idque perpendiculare ad planum quadrantis, collimando per *mo* et *pr* facile dignoſcitur, utrum perpendiculum ſit in hoc plano, per conſequens principium diviſionis *pr* ſit in plano verticali. Quod erat ſecundum.

III. Quoniam quadrans ſuſpenſus eſt duobus punctis, primum in unco *o* (Tab. V. *Fig.* 2,) deinde in cavo mutuli *n*, motum ejus verticalem nihil impedit niſi duæ cochleæ fœminæ, marem *p* complectentes. His ergo cochleis ſitus plani quadrantis ad perpendiculum corrigi poteſt, cumque hac correctione brachium horizontale quadrantis non inclinetur, ſequitur quod hæc correctio ſit independens, a ſitu horizontali quadrantis. Quod erat tertium.

IV. Uncus (Tab. V. *Fig.* 3.) non modo in *a* eſt cavus ſed ſimul eſt convexus, ut ſectio ejus *ab* ſit circularis: hæc unci figura igitur non obſtat quo minus quadrans in mutulo *n* per cochleam *h* (Tab. VI. *Fig.* 2.) nonnihil attolli aut deprimi poſſit. Proinde cum poſitio principii diviſionis dependeat ſimul a poſitione brachii horizontalis, evidens eſt, per motum cochleæ *h*, poſitionem principii diviſionis reſpectu perpendiculi corrigi poſſe.

V. Cum ipsum fulcrum ferreum habeat motum aliquem horizontalem in mutulis *ab* et *gb* (Tab. V. *Fig.* 4.) axisque rotationis sit perpendicularis, sequitur, quod reliquis omnibus manentibus deviatio plani quadrantis a plano meridici corrigi possit. Et si vel maxime axis rotationis fulcri non exacte sit perpendicularis, nihil tamen impediet, quo minus observator inclinationem hujus axis, ad quam partem ea tendat, explorare possit, et sic deinceps pro re nata correctiones suas instituere. Quod erat quintum.

VI. Quadrans ipse ex uno et solido est metallo. Quod si igitur extenditur aut contrahitur calore aut frigore, similis semper sibi manebit. Nec suspensio ejus obstat extensioni. Nam in fulcro mutulus *n* (Tab. V. *Fig.* 2.) canalem habet horizontalem, in quo cochleæ apex *h* (Tab. VI. *Fig.* 2.) hæret, et foramen *c* (*Fig.* 1.) satis amplum pro tam parva extensione aut contractione quam calor aut frigus producit, nec cochlearum fœminarum, istud foramen in facie et tergo quadrantis tegentium, brachiumque constringentium, superficies plana huic obstat. Quod erat sextum.

VII. Denique quoad rectificationem, ea jam satis ex ipsis præceptis patet. Nemo non intelliget, eam faciliorem expeditioremque esse hucusque usitatis; cumque asser pro rectificatione regulæ constructus asservari possit, quolibet tempore observator absque magno labore regulam suam denuo examinare potest. Ergo organon requisitis suis satisfacit.

Cæterum quoniam rarissime in ædificiis constructis dantur muri in plano meridiei existentes, vel saltem loca hisce muris construendis apta, arcus murales hucusque semper ædificium expresse in hunc finem, dispositum desiderarunt. Sed quilibet facile intelliget nostrum artificium quocunque ferme loco esse applicabile.

plicabile. Nam si, *v. gr.* in tali apertura quæ januarum est, duo mutuli *ab*, *gb*, (TAB. V. *Fig.* 4.) in murum immittuntur, tertius *ec*, murum contiguum præcise non requirit; sed in ferri quadam compage, longe de muri plano prominente, valide satis firmari potest.

Multa præterea sunt quæ in hoc organo aliter aut forte melius disponi possent; sed longus nimis forem omnia cogitata mea in medium hoc loco prolaturus. Sufficiat artificium scopo congruens monstrasse, idque non ingratum penitus astronomis fore confido.

XIX. *Observationes duæ* Alberti Haller, *Prof. Med.* Gotting. *R. S. S.* Lond. *S. Fabricæ morbosæ in Cadaveribus repertæ.*

OBSERVATIO I.

Read May 7. 1747.

IN fœmina quadragenaria reperi venam cavam inter renalis sinistræ originem, et inter iliacas venas, enormiter angustatam, ut vix quidquam transmitteret. Aliquid tamen polyposi duri sanguinis in ejus cavitate, quæ inter crassas membranas arctissima fuit, repertum est. Vena vero spermatica dextra enormiter dilatata, unciali diametro venæ cavæ locum subiit, et sanguinem, exclusum a via solita, reddidit ureteris venæ, alioquin in sano corpore exiguæ, ortæ ab iliaca dextra.

Rarissimi hujusmodi coalitus exemplum aliud reperi in JOHANNIS RHODII *Mantiss. Anatom. Obs.* XXI.

Ex utroque constat, etiam in maximis truncis vasorum corporis humani morbos subnasci posse, et obstructionem in venis vere dari; et canales minimos, quando sanguinis, solitis viis exclusi, impetus eo vergit, patentissimos reddi posse.

IN-

Obs. II.

IN fœmina decrepita, quam centum annorum ætate esse dictitabant, non tamen ita firma fama, ut eam sequi tuto liceat, ætatis summæ aliqua vestigia reperi.

Tota corporis fabrica durior fuit, vel cultro judice. Glandulæ conglobatæ sanæ, sed similes fere renalis carnis firmitati; nervi præduri; cellulosa tela ubique vix scissilis; costarum cartilagines nondum osseæ, nisi supremam velles, quæ cum sterno, nexu vix ullum discriminis vestigium relinquente, conferruminata erat. Sed in ea costa id non rarum est.

Verum in arteria magna multæ mortis causæ fuerunt. Amplissima primum aorta, qua ex corde prodit, ut quinque unciarum et linearum duarum esset ambitus. Deinde aperto hoc, non aneurysmate quidem, sed amplissimo tamen sinu adparuit.

1. Valvulas cordis arteriosas partim induratas, partim etiam petrosis tumoribus varias esse, perinde uti Cowperi fere figuræ demonstrant [*Myolog. reform.* T. XI.]; reliquæ valvulæ venosæ et arteriosæ cordis vix mutatæ.

2. In arteria aorta, tum ad cor, tum in thorace, in abdomine denique, membrana interna undique lacera, quasi scabendo in eminentes cristas liberas, fluctuantes, mutata, tamquam ulcere aliquo consumta esset. Hæ squamæ passim osseæ erant, alicubi etiam petrosæ, et acervi tophaceorum granorum plerorumque vasorum ex aorta oriundorum ostia obsidebant. Membrana musculosa sana fuit, tum externa, ut vitium omne in intima sederet.

3. In hypogastricis, iliacis, pelvis, arteriis, et iis quæ ex pelvi ad nates exeunt, plurimæ crustæ osseæ, subflexiles tamen, in quas mutata erat interna harum

arteriarum

arteriarum membrana, ita tamen adhærentibus fibris carneis, ut passim calculosæ squamæ ductubus transversis inscriberentur. Nihil tamen hic petrosi. In omnibus arteriis corporis prædura et figurata teretia sanguinis crassamenta, suo canali tamen minora.

4. Vesicula fellea flava bile vix amara plena, et calculis ad viginti, exiguis, angulosis, quorum unus ita obsidebat ostium ductus cystici, propius paulum cholidocho, quam prima cystici valvula, ut bilis, contra quam solet, ex vesicula premendo expelli non posset. Dulcedinem in bile, quando in calculos coivit, plerumque reperire soleo.

Hæc fere fuerunt, quæ observationem mererentur, et demonstrant arteriarum internam membranam ab ictubus repetitis cordis tandem partim indurescere, partim inter ossificata spatia rumpi, sic debilitari truncum aortæ, et ad aneurysmata reddi pronum. Demonstrat etiam in ipso sanguine ubique terram veram calculosam circumvehi, nec in renalibus solum viis deponi, sed ibi hærere et congeri, ubi ruptæ sunt levissimæ membranæ vasorum, et adtractio terrearum moleculaarum ad asperas inæquales superficies major est.

XX. *A Letter from Mr.* David-Erskine Baker *to the* President, *concerning the Property of* Water Efts *in slipping off their* Skins *as Serpents do.*

S I R, *London,* May 13. 1747.

Read May 14. 1747.

AS several little Particularities in the Production or Changes of Animals may be found of considerable Service to assist us

in

in the Knowledge of Nature, and consequently are not unworthy a Philosopher's Notice, I take the Liberty to lay before you some Observations on the common *Lacerta aquatica, Water-Lizard, Newt* or *Eft*; a Creature which most People, tho' without any good Reason, have imagined to be venomous and mischievous, and, from a groundless Aversion have avoided and neglected much more than it deserves.

The Animal I speak of is to be found in the Spring, and during the whole Summer Season, in most Ditches and shallow standing Waters throughout *England*, and is, I believe, unknown to very few; but, lest it should possibly be mistaken for any other Creature, I beg Leave to lay a Picture of what I mean before you.

When fully grown, it is about 6 Inches in Length; The Head is like that of a Frog, with a Couple of fine large Eyes: It has four short Legs, the two foremost having four Toes, and the two hindermost five, resembling the Feet and Toes of a Frog; but not at all webbed, as the hindermost Feet of a Frog constantly are: The Tail is very thin and flat, and lies not horizontally, but stands up in a perpendicular Position, and serves as a Rudder to direct it in swimming. It is amphibious, but lives mostly in the Water, wherein, tho' it can swim, it most frequently crawls about at the Bottom, rising to the Surface only now-and-then, with a wriggling Motion, to throw out a Bubble of stale Air, and take in a fresh Quantity of new. There is some Difference in their Colour, but the Back is usually of a light brown, and the Belly yellowish spotted with black Spots. In

Winter-

Winter-Time they are ſeldom or never ſeen, and therefore may be ſuppoſed to retire into Holes, and lie torpid there, as Frogs, Snakes, and many other Creatures do.

Having ſeveral Times kept theſe Animals for many Months together in Glaſs Jars, and watched them very attentively from Day to Day, I am able to ſpeak with much Certainty as to the wonderful Manner of their putting off their Skins without making the leaſt Hole or Breach therein; a Circumſtance which has occaſioned me to give you the Trouble of this Paper.

It has long been known, that moſt of the Serpent-Kind put off, or, as we commonly term it, caſt their Skins at certain periodical Times; tho' we are very little acquainted with the Manner of their performing this Work, ſince it is commonly done in their retiring Places, where we can ſeldom get a Sight of them; nor ſhould we indeed know that their Skins are changed at all, did we not often find the Skins they have caſt off. But from this little Lizard, which I have more than once carefully attended during the whole Operation, a reaſonable Gueſs may be formed as to moſt other Kinds; and as it is a Creature eaſy to procure, may be kept in a Jar of Water for many Months, and the Intervals between the Periods are ſo ſhort (for they ſhed their Skins every Fortnight or three Weeks), it is in every Body's Power to ſee with his own Eyes what I am now going to deſcribe.

A Day or two before the Skin is to be changed, the Animal appears more ſluggiſh than uſual, takes no notice of the Worms you give it, which at other times it devours greedily; the Skin in ſome Places appears looſe from the Body, and its Colour not ſo

lively as it did before; and thus it continues till the great Work of putting off the old Skin is to be performed. It begins this Work by loosening with its fore Feet the Skin about its Jaws (which, when open, are wider than any Part of its own Body) and pushes it backward gently and gradually both above and below the Head, till it is able to slip out first one Leg, and then the other; which when it has done, it proceeds to thrust the Skin backwards as far as these Legs can reach; it is then obliged to rub its Body against Pebbles, Gravel, or whatever else it can meet with, till more than half its Body is freed from the Skin, which appears doubled back, and covering the hinder Part of the Body and the Tail. When the Business is thus far done, the Animal, turning its Head round to meet its Tail, takes hold of the Skin with its Mouth, and setting its Feet thereon, by degrees pulls it quite off, the hind Legs being drawn out as the fore ones were before.

If the Skin be then examined it will be found with its Inside outwards, but not having the least Hole or Breach; that Part which cover'd the hind Legs seeming like Gloves that are turned without pulling out the Tips of the Fingers, tho' intirely perfect and unbroken. The Coverings of the fore Legs remain within the Skin. They do not however put off the Coverings of their Eyes along with the Skin, as some Snakes are found to do; for the Skin of this little Creature has always two Holes at the Places where the Eyes have been.

It is very entertaining to observe it whilst engaged in this necessary Work, which sometimes takes up near

near half an Hour, after which it appears full of Life and Vigour, as well as very ſleek and beautiful.

Theſe Obſervations have been made under the Inſpection of my Father, and ſome other curious Friends, who are Witneſſes of the Facts here mentioned. The Drawing (TAB. X. *Fig.* 1. 2.) added to this Account, and intended to repreſent one of the Animals getting rid of its Skin, may, it is hoped, aſſiſt to make the foregoing Deſcription more fully underſtood. When the Skin is come off, if it be not taken away ſoon, it is very common for the Creature to ſwallow it whole, as it does all its other Food; and if it takes in the Head-Part, as frequently is the Caſe, the Tail-Part, being filled with Air and Water, becomes like a blown Bladder, and proves ſo unmanageable that it is very diverting to ſee the Pains it coſts to diſcharge the Air and Water, and reduce it to a fit Condition to be got down its Throat.

Many Creatures of very different Kinds put off their Skins and Shells at certain Periods. All Serpents are ſuppoſed ſo to do; the Skins of ſeveral Kinds being oftentimes found whole. Crabs, Lobſters, Cray-fiſh, Shrimps, and probably moſt or all of the cruſtaceous Fiſhes, caſt their Shells from time to time; and if one may gueſs of the reſt by the freſh Water Shrimp, which I have kept ſeveral times and obſerved, their Shells are put off without any other Breach than one, longitudinally, in the Middle of the Belly Part, thro' which the Body, Tail, and Claws are pull'd out, and the Shell left in a Manner whole.

Of the Inſect Tribe, every Caterpiller has three or four Skins before its Change into the *Aurelia*

State, in which the Place of creeping out is a little below the Head. The Spider throws off the Skin or Shell three or four times, getting out of it by a Rupture underneath, and leaving every Claw, and even the horny Covering of his *Forceps* intire. Even the little Mite casts its Skin also at several short Periods, and nearly in the same manner. But I fear I have been already too tedious; and therefore begging your Pardon, subscribe myself, with all possible Respect,

SIR,

Your most obedient humble Servant,

David Erskine Baker.

A Skin of the Water Lizard cast off in the above manner waits on you herewith, as represented in (TAB. X. *Fig.* 2.) the extreme Thinness of it makes it impossible to be got out of the Water, stretched out and dried, and therefore it was necessary to be preserved in Spirits.

I beg Leave likewise to shew you one of the living Animals, as figured in TAB. X. *Fig.* 1.

A *Remark* by the *Publisher.*

WM. *Oliver*, the Viper-catcher, mentioned in No. 443. of these *Transactions*, made a Present to the *Royal Society* of a female Viper big with Young, which was kept alive in common

common green Moſs, in a Box with a glaſs Cover. She brought forth ſeveral young ones, who ſlipp'd off their Skins, and the outward Membrane of their Eyes along with them, in ſix Weeks after their Birth; and they ſhed them again two Months after: But being then put into Spirits of Wine to preſerve them, they were killed; but may ſtill be ſeen in the *Muſeum* of the *Society*. They firſt looſen the Skin about the Mouth, and ſo ſlip it off backwards, by wriggling themſelves thro' the Entanglement of the Moſs: For ſome of the Skins were torn, and Parts ſtuck in the Moſs.

C. M.

XXI. *An Improvement of the* Celeſtial Globe, *by Mr.* James Ferguſon.

Read May 14. 1747.

ON the *Axis* of the Globe, (TAB. X. *Fig.* 3.) above the Hour-Circle, is fixed the Arch *A* at one End by the Screw *D*, ſo as to leave ſufficient Room for turning the Hour-Index occaſionally: The other End at *B*, being always over the Pole of the Ecliptic, has a Pin fixed into it, whereon the Collets *C* and *B* are moveable by their Wires *F* and *G*, when the Screw *E* is ſlackned, and may be made faſt at Pleaſure by this Screw; ſo that the turning of the Globe round will carry the Wires round with it, ſhewing thereby the apparent Motions of the Sun and Moon by the little Balls on their Ends at *H* and *I*. On the Collet *C*, in which the Sun's Wire is fix'd, there is alſo fix'd the circular Plate *L*, whereon the $29\frac{1}{2}$ Days of the Moon's Age are engraved, which have their Beginning juſt below the Sun's Wire; conſequently the Plate *L* cannot be turned without carrying the Sun's

Sun's Wire along with it; by which means the Moon's Age is always counted from the Sun: And the Moon's Wire being turned so as to be under the Day of her Age on this Plate, will set her at her due Distance from the Sun for that time. These Wires being Quadrants from *C* to *H*, and from *B* to *I*, must still keep the Sun and Moon directly over the Ecliptic; because the Center of their Motions at *C* and *B* is perpendicularly over the Pole of the Ecliptic in the *Arctic* Circle. But, because the Moon does not keep her Course in the Ecliptic, as the Sun appears to do, but has a Declination of 5⅓ Degrees on each Side of it in every Lunation, she is made to screw on her Wire as far on both Sides as her Declination or Latitude amounts to. For this Purpose *K* is a small Piece of Pasteboard, to be applied over the Ecliptic at right Angles; the middle Line *o o* standing perpendicularly thereon. From this Line there is 5⅓ Degrees marked on each Side upon the outward Limb; which reaching to the Moon, makes her to be easily adjusted to her Latitude at any time.—— *N. B.* The Horizon should be supported by two semicircular Arches, instead of the usual Way of doing it by Pillars; because the Arches will not stop the Progress of the Balls, when they go below the Horizon in an oblique Sphere.

To rectify the Globe. Elevate the Pole to the Latitude of the Place; then bring the Sun's Place in the Ecliptic to the brazen Meridian, and set the Hour-Index to XII at Noon: Keeping the Globe in this Position, slacken the Screw *E*, and set the Sun directly over his Place in the Meridian; which done, set the Moon's Wire under the Day of her Age for that

that time on the Plate *D*, and ſhe will ſtand over her Place in the Ecliptic for that time, and you will ſee in what Conſtellation ſhe then is. Laſtly, faſten the Wires by the Screw *E*, and the Globe will be rectify'd.

To find the Riſing and Setting of the Sun and Moon, with their Amplitudes on the Horizon.

The Globe being rectify'd as above to the given Time, turn it round in the uſual Way, and you will ſee the Sun and Moon riſe and ſet for that Day on the ſame Points of the Horizon as they do in the Heavens. The Times of their Riſing and Setting are ſhewn by the Hour-Index, which likewiſe ſhews the Time of the Moon's paſſing over the Meridian. If you want to ſee to greater Exactneſs the Riſing and Setting of the Moon, find her Latitude for that Day by the *Ephemeris*; and as it is South or North, ſcrew her ſo many Degrees from the Ecliptic, meaſuring them by the Paſtboard *K*, applying it to the Ecliptic as above mention'd; and then turning the Globe round you will ſee the Time of the Moon's Riſing and Setting by the Hour-Index, and her Amplitude on the Horizon for that time, as it is affected by her Latitude, which will ſometimes be very conſiderable.

This may be very uſeful, eſpecially in giving Lectures upon the Globes; becauſe a large Company at ſome Diſtance will eaſily ſee this Sun and Moon going above and below the Horizon as they riſe and ſet, and likewiſe their Appulſes to different fix'd Stars: Whereas in the uſual Way, when there is only the Sun's Place in the Ecliptic ſhewn, it is not eaſy

for any one to keep his Eye upon that Part of the Ecliptic as the Globe is turned round, unlefs it be in fome remarkable Circle of Longitude; and it is not very eafy to know the Moon's Place, unlefs at her Conjunction, Oppofition, or Quadratures.

This fimple *Apparatus* fhews all the Varieties that can happen in the Rifing and Setting of the Sun and Moon, which are very curious, efpecially about the Poles, where the Sun is prefent for one Half of the Year, and abfent for the other Half; the Moon in Winter fhining conftantly without fetting from the firft to the third Quarter, in the Sun's Abfence; and in Summer the full Moon is never feen at the Poles; for fhe fets at the firft Quarter, and rifes not till the third, fave what may happen on account of her Latitude.

All the *Phænomena* of the Harveft-Moon become very plain by this additional Part; and in making fome Trials I find, that, to fome Places of the Earth, the Moon will not differ above an Hour in her Rifing for fifteen Nights together, but will differ fometimes 23 Hours in her Setting, within the Compafs of that fifteen Days; and for the next fifteen fhe will fet within an Hour of the fame Time, and differ 23 Hours in her Rifing. This is taken in round Numbers, but may be confider'd with more Exactnefs by thofe who are better acquainted with the celeftial Motions. I fhall only add, that the Places of the Earth where thefe *Phænomena* happen, are thofe lying under the Polar Circles.

XXII. *The Case of a* young Child, *at* Houghton *in* Huntingdonshire, *born with all its Bones displaced: Communicated* Febr. 8, 1746. *by Mr.* Edward Davis, *Surgeon at* Huntingdon, *to Dr.* Herman Heineken *in* London.

Read May 14. 1747.

BEING desired to see a young Child, who, they told me, was born with all his Bones displaced, I visited it, and found both the *Radius* and *Ulna* of the right Arm, with the Bones of the *Carpus* and *Metacarpus*, also the Fore-finger and Little-finger of the same Hand, all dislocated. The *Radius* and *Ulna* of the left Arm were dislocated, and receded from each other; likewise the Fore-finger and Little-Finger of the same Hand. The *Os Femoris* of the right Leg was dislocated very oddly, and laid downwards, so that one might feel the End of it: The *Patella* laid high up the Thigh; and the *Tibia* and *Fibula* at their Union with the *Os Femoris* were also dislocated, and receded very much from each other. The right Leg, the *Tibia* and *Fibula*, at their Union with the *Os Calcis*, also the *Os Calcis*, and the *Tarsal* and *Metatarsal* Bones, likewise most of the Toes. The left Leg, the *Fibula*, with some of the *Metatarsal* Bones, and and some of the Toes. The Head, upon Examination, likewise is very curious: The *lambdoidal* Suture is ossified all round, and rises with a Prominency half an Inch high: The occipital Bone has several Risings, which feel like several *Exostosis's*; and the two protuberant Sides of the occipital Bone are en-

enlarged to a prodigious Degree, and unite with each other, but leave a Dent between them which feels like a Suture. They are enlarged, I believe, to ſix Inches long, and three broad: It is all oſſified; the Midwife and Nurſe ſay it was ſoft at firſt: The reſt of the Head appears very well.

This Child is ſeven Days old: I have reduced the diſlocated Bones, tho' ſome with great Difficulty; for the Ends of the Bones and Cartilages ſeemed to be all oſſifying; and there ſeems to be an univerſal *Anchyloſis* coming on. I could not reduce the right Foot well; it was all oſſified, with the Bones diſplaced, and the *Extenſor Pedis Pollicis longus* was contracted, and had drawn the Foot almoſt round. The Jaw-Bone was alſo diſlocated, which the Midwife could eaſily put in its Place, and the Chin-Stay ſupported it very well; that is, almoſt well, only apt to ſlip out on one Side. The Midwife and Nurſe ſay, they could eaſily, for the two firſt Days, put all the Bones in their Places with Eaſe, but they continually fell out again.

The Mother receiv'd a Fall a Fortnight before Delivery, and ſhe fanſies the Bones were diſplaced with the Fall, tho' ſhe did not hurt herſelf: But whether it is from thence, or from ſome Vice in the Fluids, I ſhall not determine. If it were not for ſeveral *Exoſtoſis's* and *Anchyloſis's* in ſeveral Parts, I ſhould have imagin'd the Child (tho' ſo young) were rickety; but for the above Reaſon it cannot be that. The Child ſeems at preſent luſty and ſtrong, but I think will ſoon be otherwiſe; the Woman is luſty, and walks out about her Buſineſs, tho' but a Week ago deliver'd; and ſhe has ſix Children beſides, all very healthy.

healthy.——I intend, if the Child dies, to do what I can to get it, and make a Present of it to the *Royal Society*.

XXIII. *A Dissertation on the Situation of the ancient* Roman *Station of* Delgovitia *in* Yorkshire; *by* John Burton, *of* York, *M. D.*

Read May 28. 1747.

THE learned Antiquarians have hitherto been greatly at a Loss to find the Place where the *Delgovitia* of the *Romans* really stood; some supposing it at one Place, and some at another.

My worthy and learned Friend Mr. *Francis Drake*, in his excellent History and Antiquities of *York*, has given us every thing which has hitherto been wrote in Support of the Claim made by each Place to the Honour of rising out of the Ruins of that ancient Town; together with his Reasons for fixing that Station at *Londesburgh*; all which I beg Leave to recapitulate in as few Words as possible, and to make some few Remarks thereupon, before I proceed to shew where I think *Delgovitia* really was.

There are three Places where the Site of *Delgovitia* has been fix'd at; *viz. Weighton*, *Godmanham*, and *Londesburgh*. *See the* Map *in* TAB. X.

The Reasons offer'd for fixing it at *Weighton* are three;

First, From the supposed Derivation of the Name.

Secondly, From something like a *Tumulus* being at the East End of the Town. And

Thirdly, Becauſe the Diſtance from *Derventio* agrees with the Itinerary.

As to the firſt, the learned *Cambden* would have *Delgovitia* to be derived from the *Britiſh* Word *Delgwe*, which ſignifies the Statues or Images of the Heathen Gods; therefore, as ſome Perſons would have *Weighton* to be derived from ſomething of the like Cauſe, upon no other Foundation, than that *Weightelberg* in *Germany* is noted by *Conrad Celtes*, ſays Dr. *Gale*, as a remarkable Town in thoſe Parts.

Another Perſon (*a*) ſays, that *Delgovitia* was called *Devovicia*, or *Delvovicia*; from whence take *Vic*, and add the *Saxon* Termination *Ton*, there is ſomething like *Wighton*; eſpecially when we conſider that the *Saxon* U and W were ſounded alike.

Both theſe (ſuppoſed) Proofs for *Weighton* will drop, when we conſider, that an eaſier and much more probable Derivation of it may be found out; the Name being intirely *Saxon*, and is plainly derived from the *Saxon* Word ƿeᵹ, or ƿæᵹ, *Via*, *Stratum*, a Road or Street; and from the Verb ƿeᵹan, *ire*, *tranſire*, to travel: The Termination *Ton* is obvious to all; ſo the *Belgic* or *High-Dutch Wech*, *Wegh*, *Weghe*, are the ſame with our *Way*, and ſignify the ſame Thing. This is moſt likely, becauſe *Weighton* now ſtands at the Conjuncture of ſeveral Roads, which here meet, and run from thence by *Kexby-Bridge* to *York*, and thence may be called *Weighton*, or *Way-Town*. The *Roman* military Ways, both from *Prætorium* and from *Lindum*, took

(*a*) In the Chorography of *Britain*.

a different Courſe, and went by *Londesburgh*, as I ſhall ſhew in the Sequel; and the old Road being turn'd this Way, a new Town ſprung up, which took its Name from the Occaſion of altering the Road.

The ſecond Reaſon offer'd to prove *Weighton* to have been *Delgovitia* is, becauſe Mr. *Horſeley* obſerved ſomething like a *Tumulus* at the Weſt End of the Town. This alone is ſo weak an Argument to prove this Place a *Roman* Station, that I ſhall take up no Time in endeavouring to confute it. I muſt alſo remark, that there are not the leaſt Remains of any *Roman* Road leading to *Weighton*; which there always are to any known *Roman* Station.

The third Reaſon offer'd, is, becauſe this Place agrees ſo near with the Diſtance from *Derventio*, as mention'd in the Itinerary; but this Argument muſt fall, when I ſhall ſhew in the Sequel, that it is not true in Fact, having had the Road meaſured.

There is no other Reaſon offer'd, why *Delgovitia* was where *Godmanham* now is, than that this latter Place was ſaid, by Venerable *Bede*, to be *Locus Idolorum*, or a Place of Idols. Mr. *Burton*, in his *Itin. Anton.* ſeems to lay a Streſs on the *quondam Idolorum Locus*, and ſays, It may allude, as well to *Roman* Idols as *Saxon*: But this is too far ſtrain'd; and we may juſtly enough conclude, that this was a Temple neither of *Roman* Structure nor Worſhip, but a Place dedicated to *Saxon* Idolatry; ſuch a one as is deſcribed in *Verſtegan*, incloſed with a Hedge inſtead of a Wall: For we find the curious Mr. *Drake* made a very ſtrict Enquiry, and could not perceive the leaſt Remains of any Ruins; meeting with nothing

thing but Holes and Hills, where Chalk or Lime-Pits had been made.

There being not sufficient Proof offer'd to fix *Delgovitia* here, I shall now proceed to examine what Mr. *Drake* brings, to prove that *Londesburgh* may claim this Honour, he being the first Person who has attempted to shew it; and I must own, has brought much more substantial Reasons to support his Opinion, than have been mention'd in Behalf of either *Weighton* or *Godmanham*. These are five in Number; *viz.*

First, From the Name of the Place, *i. e. Burgh* or *Brough*.

Secondly, The Distance from *Eboracum* and *Derventio* will answer the calculated Miles in the *Itinerary*, as well as *Weighton*.

Thirdly, Because the *Roman* Road lay that Way.

Fourthly, Because *Roman* Coins are found there. And

Fifthly, Because there are Repositories for the Dead often found.

As to the first; That it might derive Part of its Name, *viz. Burgh*, from a Fortress on Land, I agree to; and very likely there might have been some Guards kept there; because it stands so high, that it commands the Prospect of the whole Country from thence to *Brough*, where the *Romans* used to ferry over to and from *Lincoln*; as appears by the military Road on both Sides the River. Here might, I say, have been a Guard kept, to see that no Enemy came on that Side to surprise them in their Station; and which indeed seems to be the more confirmed by the Coins found here, as well as the Number

of human Bones. Tho' this laſt is no certain Proof; becauſe a Battle or Skirmiſh might have been fought near or upon this Place; which indeed ſeems to have been the Caſe, ſo many Bodies being found together; yet, when join'd with other Circumſtances, it helps ſtrongly to confirm his Opinion. But I have not heard of any old Foundations, Ruins, or *Roman* Pavements having been diſcover'd in or near this Place.

The ſecond Reaſon offer'd is, becauſe it will anſwer the calculated Miles in the *Itinerary*. This Place comes nearer the *Itinerary* than *Weighton*; but this Point I ſhall wave diſcuſſing, till I come to treat of that Place where I think *Delgovitia* ſtood.

The third Reaſon is, becauſe the military Way led thither from *Brough*. This, tho' a Proof that the *Romans* paſſed and repaſſed where that Place now ſtands, yet is no Proof of its being a *Roman* Station.

The fourth Reaſons I have already ſpoke to under the firſt Head here offer'd; ſo I ſhall now proceed to prove, I hope, where *Delgovitia* really ſtood.

I ſhall not ſpend much Time in ſpeaking of the Wiſdom of the *Romans*, either in their military or political Capacity: Some few Things however, tending that Way, I am obliged to mention, in order to ſhew their Motives for fixing a Station in the Place I hint at, and then I ſhall endeavour to bring my Proofs that that Station was their *Delgovitia*.

Among others, theſe were ſtrong Reaſons for keeping the ſeveral Stations; *viz.* to guard the Paſſes, to keep the Country in Awe, and to have a conſiderable Number of Men together, to prevent a Surprize from any foreign Invaſion, and ſometimes to ſecure a Supply of Proviſion.

I don't

I don't know any Part of the County of *York* that required so strong a Guard as this Place (strong it has been, is evident from the Plan of the Camp); because it is so situated, that *York*, for want of this Station, might have been sooner surprised, either by any foreign Enemies, who might have made a Descent upon them, from either the *Humber*, or *German* Ocean; or from an Insurrection of the Inhabitants of the *East-Riding*; both which are by this Station sufficiently guarded against. It likewise gave them an Opportunity of receiving and defending their Recruits, either from *Rome*, or from any other of the Southern Parts of the Island; who could either come by Water, or cross the River from out of *Lincolnshire*.

As the guarding their Passes was one strong Reason for fixing Stations, any Person, by looking on and examining the annexed Map, (*see* TAB. X.) and the Draught of the Camp, (*see* TAB. IX.) will easily at one View, see why that Place must be preferable for such a Station; for it is just at the Angle where four *Roman* military Ways meet; so that, by guarding sufficiently that one Pass, they secur'd all the four Roads; which, had they been station'd either at *Weighton*, *Godmanham*, or *Londesburgh*, they could not with the same Men have done. This will be more evident, by drawing a Line of the *Roman* Road on the Map of *Yorkshire* from *Brough Ferry* on the River *Humber* thro' *Londesburgh* Park to *Malton*, and from *Stamford-Bridge* to *Pattrington* near *Spurnhead*; and then you will find that the Roads cross each other betwixt or near *Millinton* and *Wartre* Priory.

Moreover the Country itself, by Nature, helped, with only little Art, to make their Camp at that time

time almoſt impregnable, the Hills being from 60 to 90 Yards perpendicular in Height, and their Sides very ſteep, which are very apparent in the Plan.

From one Part of this Camp they could ſee a great way towards *Malton*, and all the Way down the great Vale of *York*, from near *Hambleton-Hills* to *Howden*, and from *Londesburgh* (where I doubt not but they kept a Watch-Guard) they could ſee all the Way from *Howden* to *Brough Ferry*. Hence they could not be ſurpriſed from the South-Eaſt, South-Weſt, or North-Weſt Quarters; therefore they had only chiefly the Eaſtern Sides to fortify? and how they have done that, the Plan of their Camp (Tab. IX.) will better ſhew than I can deſcribe.

Another Reaſon for their fixing here is very evident; becauſe, at the Foot of the Hill, not 100 Yards from where the *Roman* Pavement was, there are two Springs of fine clear Water, which, united, form what they call the *Beck*. Theſe Springs never fail, even in the hotteſt and drieſt Summers (a rare thing to be met with upon the *Wolds*) and there is not another Spring within two Miles of that Place, but what is either quite dried up, or greatly diminiſhed in a dry Seaſon; inſomuch that at this Day, in ſome Seaſons, the People are obliged to drive their Cattle ſeveral Miles hither for Water.

From this Situation their Army could never want Proviſions, having a free Communication either by Land or Water, with the Southern Parts of the Iſland.

All that Part of the Plan of the Camp (Tab. IX.) marked *a. a. a. a.* deſcribes deep Valleys; from the Bottom of which to the Top of the Hills are in general

 from

from 60 to 90 Yards in perpendicular Height, and the Sides are very ſteep. All along the Hills, from Vale to Vale are *Roman* Works, repreſented in the Plan at *b.b.b.b.*; ſo that nothing could paſs that Way, without the Knowledge and Conſent of the Guards. It muſt alſo be obſerved, that, of all the Works, thoſe guarding the Parts toward *Bridlington* are the ſtrongeſt; they being from 4 to 6 Ditches in Breadth, each of which are 10 or 12 Yards broad.

At the Places marked *c.c.c.c.* in the Valleys, were Watch-Guards, kept to prevent any Surprize, by the Enemy attempting to get at the Station that Way.

All theſe Works incloſe 4185 Acres of Ground; whence it is evident here muſt have been a large Army. You ſee in ſeveral Places where their *Tumuli* or Barrows were, repreſented by little green Hills.

Having ſhewn the Fortifications and Out works of the Camp, I will now prove the Part within theſe on which *Delgovitia* ſtood.

About half a Mile North-Eaſt of *Millington*, on the South Side of a gently ſloping Hill, were found ſeveral Stone Foundations of Buildings of different Sizes, and of different Shapes; among which were found ſeveral Fragments of *Roman* Pavements, *Roman* Tiles, Flues, and two *Roman* Coins, all or the chief of which are repreſented in their reſpective Colours in Tab. IX. and X. Theſe are all Proofs of the Buildings having *Roman*. There was likewiſe dug up a Piece of a large Stone Pillar, of about ſix Feet in Length, but of no regular Order; which, notwithſtanding, might yet be *Roman*; for we can't ſuppoſe thoſe military People

People ſo well skill'd in Architecture as the Artiſts at *Rome*.

If *Delgovitia* (as *Cambden* hints) be derived from the *Britiſh* Word *Delgwe*, which ſignifies Statues or Heathen Gods, this Place may lay claim to a Title on that Account, much ſooner than either *Weighton* or *Godmanham*; for here was dug up a circular Foundation reſembling a Temple in all Appearance; being within of 45 Feet Diameter, and the Foundation was near five Feet thick.

Near to this circular Building, but South of it, were the Foundations of two oblong ſquare Building, but with a ſtrait Entrance, not two Feet wide, wherein I apprehend they put in the Fuel and Fire for their Sacrifices; there being evident Marks of burning upon the Stones, they being almoſt burnt thro': Moreover, in digging in the middle of theſe two Buildings, we found about half a Yard thick of Aſhes, wherein were ſome few ſmall Pieces of Wood, Fuel, and Pieces of Brute-Bones, chiefly burnt, and a great Part of an Horn of a large Deer, which I now have by me.

Eaſt of theſe were laid open the Foundations of another ſquare Building, wherein we found the Pavements, Coins, *&c.* as are here repreſented in Tab. X.

The Situation of theſe Buildings was very ſtrong, being guarded on the South-Eaſt and North-Eaſt by a deep Vale, the Sides of which are very ſteep.

From what has been ſaid, I think there is nothing wanting now to prove this to have been the *Delgovitia*, but to reconcile the Diſtance as mention'd in the *Itinerary*.

From *Eboracum* to *Derventio*	*M. P.*	VII.
From *Derventio* to *Delgovitia*	*M. P.*	XIII.
	Total *M. P.*	XX.

I had the Road meaſured from *York* to the circular Building or Temple, and the Particulars were as follows; *viz.*

	Miles.	Yards.
From *York* to *Stamford-Bridge*, or *Derventio*, were — — —	7	154
From *Stamford-Bridge* to the firſt Beginning of the *Roman* Works, — —	7	209
From the firſt Barrier to the circular Foundation, — — — —	3	132
From the Temple to the Eaſt Side of the Works is — — —	$2\frac{1}{4}$	0
Total.	$17\frac{1}{4}$	55

This, tho' not exactly the ſame Diſtance mention'd in the *Itinerary*, is yet nearer it than either *Londesburgh*, *Weighton*, or *Godmanham*; for *Londesburgh* is five computed Miles farther than this Place; and if we add 1 Mile and a half more (they in general meaſuring one Third more than computed at), then it will be about 6 Miles and a half; which, added to 17 Miles and a Quarter, will make *Londesburgh* to be 23 Miles and three Quarters from *York*; which differs more from the *Itinerary* than the Place where I ſuppoſe the *Delgovitia* to have been. Both *Godmanham* and *Weighton* are ſtill farther off, the laſt being 3 computed Miles from *Londesburgh*;

desburgh; and if it measures one Third more, then it will amount to 4 Miles and a half; which, added to 23 Miles and three Quarters, will make the Distance betwixt *Eboracum* and *Weighton*, by *Derventio* and *Londesburgh*, to be 28 Miles one Quarter.

I think this little Variation from the *Itinerary* not to be an Argument strong enough to prove this Place not to have been the *Delgovitia*, when put in Competition with the Situation, *&c.* which at one View both of the Map and Plan will appear; considering at the same time, that the Proportion betwixt the *Roman Mille Passuum* and our Miles is nearly as 19 to 21.

From what I have said, I think it is evident, that neither *Weighton*, *Godmanham*, nor *Londesburgh*, stand where *Delgovitia* was. I have in the first Place shewn the Probability of this Place near *Millington* being the Station, from the known Prudence of the *Romans*, because one Set of Men could defend the whole four Passes; which could not have been done, had they been placed at *Weighton*, *Godmanham*, or *Londesburgh*.

Secondly, I have shewn, that, from the very Situation and Nature of the Country, there required but little Art to make their Camp, at that time, almost impregnable; the Valleys in general being from 60 to 90 Yards deep, and the Sides thereof very steep.

Thirdly, That from this Camp and *Londesburgh* they might see the whole Country from the *Humber* on the South-East, up the Vale of *York* on the West towards the North-West Side; so that no Army could surprise them that Way.

Fourthly, That they could always have a sufficient Quantity of Provisions, and never want Water, even in the hottest Summers. And,

Fifthly, That there has been a *Roman* Station here, as is evident from the *Roman* Pavement, Coins, Tiles, and Foundations of the Ruins: And if the *Romans* had had a Station at *Weighton*, *Godmanham*, or *Londesburgh*, they would scarce have had one so near the other.

All these Things concur in proving this to be the Site of *Delgovitia*; and there is or can be no Argument brought against it; except that, by the *Itinerary*, the Distance from *Eboracum* by *Derventio*, is set down at XX *M.P.* and by our Measure the Distance from *York* to the circular Foundation, in the Camp, is only 17 Miles, one Quarter, and 55 Yards; so that there is above two measur'd Miles and one half Difference. In Answer to this, I say, May not the *Itinerary* be as wrong here as in some other Places; which is very evident in several Instances? And as it is wrong in some others, I doubt not but it may be so in this: Besides, the *Romans* might calculate from the Center of *York*; and this Mensuration only goes from the Bar at *Walmgate* to the circular Foundation in the *Roman* Camp. But supposing the *Itinerary* to be exactly right, yet, when the Difference betwixt the *Roman Mil. Pass.* and our Miles is calculated, I believe it will end all Disputes on that Score.

An Appendix *to the foregoing Paper, by Mr.* Fr. Drake *F. R. S.*

TIME, which ſubverts and deſtroys the greateſt Works of Mankind, hath an equal Property of bringing Things to Light. The DELGOVITIA of the *Romans* in this Country, ſo long ſought after by *Cambden*, and other Writers, as well as myſelf, is at length diſcover'd ſo far, that there is no need of any more Conjecture about it.

Being informed, in the Year 1745, of ſome *Roman* Curioſities found in a Field near *Millington*, on the *Wolds*, Dr. *Burton* of *York* and myſelf ſet out to ſurvey them. On our coming to the Place, an intelligent Countryman and his Father conducted us to a large plain Field, on the South Side of *Millington* Wood, where we were ſhewed ſeveral Foundations of Buildings under Ground, on the very Stones of which the apparent Marks of Fire may be traced. Two Baſes of Pillars, of an irregular Order, and a large Piece of a Column, were alſo diſcover'd; ſeveral Pieces of teſſelated Pavements, *Roman* Bricks, Tiles, *&c.* were dug up. The Father told us, that, about 40 Years before, he ſaw the Foundations of a circular Building, about fifteen Yards Diameter, dug up in this Place; which muſt have been the Veſtiges of ſome *Circûs* or Temple: That it had been the Cuſtom for the Inhabitants of their Village, Time out of Mind, to dig for Stones in this Ground when they wanted; and that they muſt often do, in a Country almoſt clear of ſuch Materials. The Church of *Millington* itſelf ſeems to have been built out of the Ruins of this antient *Roman* Station.

That this was really the DELGOVITIA ſo long ſought after, I think, is beyond Contradiction. The Diſtance from *York* coincides very juſtly with the *Itinerary*; nineteen or twenty *Italian* Miles agrees pretty well with our preſent Computation; and at the ſame time points out the true military Way from the *Humber* to *York*. Inſtead of forcing a Road through the Vale, the *Romans* very wiſely choſe to mount

mount the Hills as ſoon as poſſible; and therefore directed their *Stratum* from *York* to the Ford, over the River *Derwent* at *Stainfordburg*; and from thence in a direct Line to *Garrowby* Hill; which I take to be corrupted from *Barrowby*, many of thoſe *Tumuli* or *Barrows* being near this Place. On the Top of this Mountain, as I may well call it, though the Road turns up it by an eaſy Aſcent, begins a Series of ſuch enormous Works for Fortification, as the like is not to be met with in the whole Iſland.

This Road on the Summit of the Hill in a ſtrait Line points directly for *Sureby* or *Burlington-Bay*, the SINUS SALUTARIS of *Ptolemy*. But another Road to the right takes a different Courſe, and comes down to the Ruins I have before mentioned. From thence the Road leads directly to *Londesburg*, the Place which I once thought the Station ſought for: It paſſes thro' Lord *Burlington*'s Park, where more of it was laid open laſt Year than I had before ſeen, in widening the large and noble Canal in that Incloſure. This Place was before a Moraſs, and the *Romans* were obliged to force a Way through it, which is eight Yards broad, and laid with Stone edgeways to a great Depth. The Road paſſed up the Hill on the other Side this marſhy Place, and divided into two Branches on the Top of it; one Way pointing through *Weighton* to *Brough* on the *Humber*, and the other by the Eaſt End of *Godmondham* directly for *Beverley*; which now I am convinced alſo was the PETVARIA of *Ptolemy*. From which laſt Station it muſt have gone out directly for *Patrington* or *Spurnhead*; one of which was certainly the *Roman* PRAETORIVM, mention'd as the laſt Stage in the firſt itinerary Route of *Antoninus*.

This Sea-Port muſt be very commodious to touch at, either going or returning from *Gaul*, or the *Belgic* Coaſts, and bringing military Stores, *&c.* from thence, either to *York* or *Malton*; to which laſt Place the CAMOLODVNVM of *Ptolemy*, another Road branches out, apparently from the Conjunction on the Top of *Garrowby-Hill*, and leads directly to it. But to return to our DELGOVITIA.

The

The Situation of this Place is admirable, and the ſtupendous Works about it, thrown up for a Defence to this Station, and the ſeveral grand Roads near it, are not to be deſcribed. The Town itſelf was placed on a Declivity of a Hill, almoſt full South; and very near its Ruins ariſe ſome rapid Springs of excellent Water; and ſo copious, as, when joined in one Stream, turns a Mill; from which I ſuppoſe the Name of *Millington* has proceeded. There was alſo lately diſcover'd a Well above a Mile E. *b* S. from theſe Springs, dug thro' the ſolid Rock, Twenty-ſix Yards deep, which muſt have been a *Roman* Work.

To the South-Weſt there are no Ramparts thrown up; but to the Eaſt, North-Eaſt, and due North, the whole Country is full of them. The Vales are all of them guarded by ſmall Encampments at their Angles; the Veſtiges of the Barracks, now viſible, are called by the Country People the *Camps*. Theſe were to prevent any ſudden Surprize that Way. On the Hills, from Vale to Vale, ſome of which are from 60 to 90 Yards deep, and prodigious ſteep, are thrown up Works, as Ramparts, 12 Yards broad, and proportionably high, which join in right Angles with the Vallies, and ſerve as a ſtrong Barrier everywhere. Whether they had any Paliſadoes upon them is uncertain: but without them they are capable of ſtopping an Enemy; which they had Cauſe to be apprehenſive of, by its Neighbourhood to the *German Ocean*, from whence the *Saxons* gave them many Alarms.

Dr. *Burton* was at the Expence to have the Whole of theſe Works meaſur'd and plann'd out; a Map of which has been exhibited to the *Society* (*ſee* TAB. IX.); but, upon my ſhewing a correct Copy of the Draught to Lord *Burlington*, his Lordſhip order'd me to get the Survey of the Road leading from thence through his Park at *Londesburgh* to the Diviſion into two added to the former Plan; by which a Courſe of ſome

Miles, and many Thousands more Acres on both Sides the Road are given.

F. Drake.

XXIV. *A Letter from Mr.* Joseph Ames *F.R.S. and Secr. of the* Soc. Antiquar. *to* C. Mortimer *M. D. Secr. R. S. concerning a* Plica Polonica.

Good Sir,

Read May 28. 1747. *JUNE* the 22d, 1746, in the Morning, Mrs. *Hannah Coomes*, a neat old Woman, whose Hair (or *Plica Polonica*, as it is call'd) I shew'd the *Society* last *Thursday*, came and gave me the following Informations.

That she was of a genteel Family in *Staffordshire*, who had suffer'd much in the Civil Wars; and that her Mother had her Hair grow in the same manner, whose Maiden Name was *Alice Goldsmith*; but her own Maiden Name was *Hannah Bunby*, born in the Hay-market, in the Parish of *White-Chapel* and baptized at *Aldgate* on a *Saturday* the of *June*, 1645. Her Mother, having such sort of Hair, used to comb hers much to prevent it, till sometimes the Blood came: When she was about 14 Years old she perceived it to grow thick just about the back Part of her Head, and at length grew to this matted long Substance I now saw it, of 109 Inches long. She says

ſays ſhe has had four Husbands; the firſt *Nicholas Woodcock*, to whom ſhe was married when about 28 Years old, and had four Children by him; all died young; but obſerved nothing of their Hair growing ſo. I am,

S I R,

Your moſt humble Servant,

J. Ames.

See ſome Caſes of the *Plica Polonica* in theſe *Tranſactions*, No. 417. *p.* 50. *et ſeq.*

C. M.

XXV. *A Deſcription of ſome* Clay Moulds *or Concaves of ancient* Roman *Coins found in* Shropſhire; *by Mr.* Henry Baker *F.R.S.*

Read June 4. 1747.

HAVING been lately favour'd by a very ingenious Gentleman of *Shrewsbury* with the Sight and Uſe of ſome ancient Moulds made of Clay, bearing the ſame Types and Inſcriptions that ſome of the *Roman* Coins are known to have, and being enabled, by the Aſſiſtance of our moſt worthy *Preſident* of the *Royal Society*, to make out the Inſcriptions and Types impreſſed on the ſaid Moulds, I judged it might prove agreeable to this *Society* to ſee Specimens of ſo great a Curioſity as theſe Moulds ſeem to be, and to have ſome Account concerning them.

 Four

Four of the five in my Possession were found in digging Sand, at a Place called *Ryton* near *Condover* (query whether *Rutunium*), 5 Miles from *Wroxalter* (i. e. *Uriconium*) in *Shropshire*, about a Mile from the *Watlingstreet* Road: These are all of the Size of a *Roman Denarius*, and little more than the Thickness of our Halfpenny. They are made of a smooth Pot, or rather Brick-Clay, that seems to have been well cleansed from Sand or Dirt, and well beat or kneaded, to render it fit for taking a fair Impression. Great Numbers of these were found, but, for want of Care, most of them were broke in Pieces. They are described N°. 1, 2, 3, 4, in the Account below. The fifth, which is twice as thick as any of the rest, was found at *Wroxalter*; the Clay it is made of differs but little from the former; and the Impression on it is also of the Size of a *Denarius*, and of the same Time with the rest.

See TAB. X.

N°. 1. Is probably the Reverse of a *Denarius* of *Severus*, mention'd in *Mezzobarba*, p. 268. from *Noris de Vot*. X.—*Figura velata coram ara sacrificans.*— VOTA. SVSCEPTA. X.

N°. 2. On one Side — *Caput Juliæ Severi.*— IVLIA AVGVSTA.
On the other Side is the Reverse of a *Denarius* of *Severus*. *Mezzob*. p. 274. — *Victoria gradiens cum fune super scuto.* — P. M. TR. P. VIII. COS. II. P. P.

N°. 3. A Reverse of *Caracalla*. *Mezzob*. p. 286.— *Trophæum de Parthis cum duobus captivis assidentibus.* — PART. MAX. PON. TR. P. V. COS.

N°. 4. *Caput Juliæ Severi.* — IVLIA AVGVSTA.

N°.

N°. 5. *Caput Juliæ Severi.*——IVLIA AVGVSTA.

I remember no Account of any ſuch kind of Moulds being found in other Countries, excepting ſome ſaid to be found at *Lyons*; but I believe more of them have been diſcover'd at different Times in *England*; though I cannot pretend to ſay when, or where. I have been informed, that ſome Years ago, the Earl of *Winchelſea* had ſeveral Impreſſions or Moulds of this Sort (all joined together Side by Side) on one flat Piece of Clay, as if for the making many Caſts at once: They were all of the Emperor *Severus:* And I have ſeen, in the Earl of *Pembroke's* moſt valuable Collection a Clay Mould impreſſed on both Sides, as N°. 2. amongſt theſe alſo is, one of the Sides bearing the Head of the ſame Emperor, and the other Side a known Reverſe of his. Four of the Five in my Hands are alſo of *Severus* or his Wife *Julia*, and the other is a Reverſe of *Caracalla*, his Son and immediate Succeſſor; ſo that all, we know of, may be ſaid to be of the ſame Time very nearly.

They are ſeemingly intended for the Coinage of Money; though it is very difficult to conceive in what manner they could be employed to that Purpoſe; eſpecially N°. 2. which has an Impreſſion on both Sides; unleſs we ſhould ſuppoſe they coined two Pieces at the ſame time, by the Help of three Moulds, of which this was to be the middle one.

If, by diſpoſing theſe into ſome ſort of Iron Frame or Caſe (as our *Letter-Founders* do the braſs Moulds for caſting their Types) the melted Metal could be poured into them, it would certainly be a very eaſy Method of coining, as ſuch Moulds require little

Time

Time or Expence to make, and therefore might be ſupplied by new ones, as often as they happen'd to break.

Theſe Moulds ſeem to have been burnt or baked ſufficiently to make them hard, but not ſo as to render them porous like Bricks whereby they would have loſt their ſmooth and even Surface; which in theſe is plainly ſo cloſe, that whatever Metal ſhould be formed in them would have no Appearances like the Sand-Holes, by which counterfeit Coins or Medals are uſually detected.

London, May 20.
1747.

XXVI. *An extract of a letter from* William Jones *Eſq; F. R. S. to* Martin Folkes *Eſq; Preſident of the* Royal Society; *containing a commodious diſpoſition of equations for exhibiting the relations of goniometrical lines.*

THEOREM.

Preſented July 4. 1747.

IN a circle whoſe radius is r, *let there be two arcs,* A *the greater,* a *the leſs, each in the firſt quadrant; put* s, t, ſ, *and* v, *for the ſine, tangent, ſecant, and verſed ſine of an arc;* s', t', ſ', *the ſine, tangent, ſecant of the complement, and* v', *the verſed ſine of the ſupplement of that arc; let* $z = \frac{1}{2}A + a, x = \frac{1}{2}A - a$; *or if* z *and* x *be put for the arcs, it will be* $A = z + x, a = z - x$.

Then will the terms in any column of the following table, be proportional to their correſponding ones in any other column.

From

A TABLE of the Relations of Goniometrical Lines.

$2s,z$	$\frac{s,2z}{s,\overline{A+a}}$	$\frac{v\,2z}{v\overline{A+a}}$	$s,A+s\,a$	$\grave{s},a-\grave{s},A$ $vA-v\,a$
$2s,x$	$s\,A-s,a$	$\grave{s}\,a-\grave{s}\,A$ $vA-v,a$	$\frac{s\,2x}{s\,\overline{A-a}}$	$\frac{v,2x}{v\overline{A-a}}$
$2\grave{s},z$	$\frac{\grave{v},2z}{\grave{v}\overline{A+a}}$	$\frac{s,2z}{s,\overline{A+a}}$	$\grave{s},A+\grave{s},a$	$s\,A-s,a$
$2\grave{s},x$	$\grave{s},A+\grave{s},a$	$s,A+s,a$	$\frac{v,2x}{\grave{v}\overline{A-a}}$	$\frac{s,2x}{s\,\overline{A-a}}$
r	$\grave{s},z$	s,z	$\grave{s}\,x$	s,x
$\text{ſ},z$	r	t,z		
t,z	s,z	$\text{ſ},z-\grave{s},z$		
$\grave{\text{ſ}},z$	$\grave{t},z$	r		
$t,r+\grave{t},z$	$\grave{\text{ſ}},z$	$\text{ſ},z$		
$\text{ſ},x$			r	$\grave{t},x$
t,x			s,x	$\text{ſ},x-\grave{s}x$

From hence, almoſt an infinite number of theorems may eaſily be derived; ſome of which are the following, given here as examples of the uſe of the table.

I. $s,z\times s,x=\frac{1}{2}r\times\overline{\grave{s},a-\grave{s},A}=\frac{1}{2}r\times\overline{\grave{s},\overline{z-x}-\grave{s},\overline{z+x}}=\frac{s,z}{\grave{\text{ſ}},x}rr=\frac{s,x}{\grave{\text{ſ}},z}rr.$

$\grave{s},z\times\grave{s},x=\frac{1}{2}r\times\overline{\grave{s},a+\grave{s},A}=\frac{1}{2}r\times\overline{\grave{s},\overline{z-x}+\grave{s},\overline{z+x}}=\frac{\grave{s},z}{\text{ſ},x}rr=\frac{\grave{s},x}{\text{ſ},z}rr.$

II. If A, B, C, be any three angles; $Z=A+B, X=A-B, H=\frac{1}{2}\overline{A+B+C}.$

Then $\frac{1}{2}r\times\overline{v,C-v,X}=s,\frac{1}{2}\overline{C+X}\times s,\frac{1}{2}\overline{C-X}=s,\frac{1}{2}\overline{A+C-B}\times s,\frac{1}{2}\overline{B+C-A}=s,\overline{H-B}\times s,\overline{H-A}.$

And $\frac{1}{2}r\times\overline{v,Z-v,C}=s,\frac{1}{2}\overline{Z+C}\times s,\frac{1}{2}\overline{Z-C}=s,\frac{1}{2}\overline{A+B+C}\times s,\frac{1}{2}\overline{A+B-C}=s,H\times s,\overline{H-C}.$

III. $\frac{ss,z}{\grave{s}\grave{s},z}=\frac{tt,z}{rr}=\frac{rr}{\grave{t}\grave{t},z}=\frac{v,2z}{\grave{v},2z}=\frac{t,z}{\grave{t},z}$; Or $\frac{ss,\frac{1}{2}z}{\grave{s}\grave{s},\frac{1}{2}z}=\frac{tt,\frac{1}{2}z}{rr}=\frac{rr}{\grave{t}\grave{t},\frac{1}{2}z}=\frac{v,z}{\grave{v},z}=\frac{t,\frac{1}{2}z}{\grave{t},\frac{1}{2}z}$

IV.

IV. $\frac{1}{2}r = \frac{ss,z}{v,2z} = \frac{ss,\frac{1}{2}z}{v,z} = \frac{\grave{s}\grave{s},z}{\grave{v},2z} = \frac{\grave{s}\grave{s},\frac{1}{2}z}{\grave{v},z}$; and $s,z = \frac{2ss,\frac{1}{2}z}{t,\frac{1}{2}z} = \frac{2\grave{s}\grave{s},\frac{1}{2}z}{\grave{t},\frac{1}{2}z}$.

V. $\frac{s,z}{v,z} = \frac{r}{t,\frac{1}{2}z} = \frac{\grave{t},\frac{1}{2}z}{r} = \frac{\grave{v},z}{s,z}$.

VI. $\frac{t,z}{t,x} = \frac{s,A+s,a}{s,A-s,a} = \frac{\grave{t},x}{\grave{t},z}$; and $\frac{rr}{t,z\times t,x} = \frac{\grave{t},z}{t,x} = \frac{\grave{t},x}{t,z} = \frac{\grave{s},a+\grave{s},A}{\grave{s},a-\grave{s},A} = \frac{\grave{t},z\times\grave{t},x}{rr}$.

VII. $\frac{s,A}{s,a} = \frac{t,z+t,x}{t,z-t,x} = \frac{s,\overline{z+x}}{s,\overline{z-x}}$; if z and x are two arcs, then $A=z+x$, $a=z-x$.

VIII. $s,\overline{z\pm x} = \frac{s,z\times\grave{s},x\pm\grave{s},z\times s,x}{r} = \frac{t,z\pm t,x}{\int,z\times\int,x}$.

IX. $\grave{s},\overline{z\pm x} = \frac{\grave{s},z\times\grave{s},x\mp s,z\times s,x}{r} = \frac{rr\mp t,z\times t,x}{\int,z\times\int,x}r$.

X. $t,\overline{z\pm x} = \frac{t,z\pm t,x}{rr\mp t,z\times t,x}rr$; and $\grave{t},\overline{z\pm x} = \frac{rr\mp t,z\times t,x}{t,z\pm t,x}$.

XI. $\int,\overline{z\pm x} = \frac{\int,z\times\int,x}{rr\mp t,z\times t,x}r$; and $\grave{\int},\overline{z\pm z} = \frac{\int,z\times\int,x}{t,z\pm t,x}$.

XII. In three equidifferent arcs A, z, a; where $z\ (=\frac{1}{2}\overline{A+a})$ is the mean arc, and $x\ (=\frac{1}{2}\overline{A-a})$ their common difference; put $p=\frac{\grave{s},x}{r}$, $q=\frac{s,x}{r}$; $P=2p\times s,z$, $Q=2q\times\grave{s},z$.

Then $s,A = P - s,a = Q + s,a$; And $s,a = P - s,A = s,A - Q$.

XIII. Let $d = v,A - v,a = \grave{s},a - \grave{s},A$; then $ss,A - ss,a = \overline{2\grave{s},A+d}\times d = \overline{2\grave{s},a-d}\times d$.

Note, When an arc is terminated in the second, third, or fourth quadrant, some of the signs (+ and −) of the terms in the preceding theorems, will, by the known rules, become contrary to what they now are.

XIV.

XIV. Let A, B, C, &c. be the sines, a, b, c, &c. the co-sines, a', b', c', &c. the tangents, of the arcs, α, β, γ, &c. whose number is n; the radius being r; put S for the product of the n co-sines, S', S'', S''', &c. for the sum of the products made of every sine, every two, three, &c. sines, by the other ($n-1$, $n-2$, $n-3$, &c.) co-sines, where the co-sine noted by $n-n$ is unity.

Then the sine of $\overline{\alpha+\beta+\gamma+\delta, \&c.} = \overline{S - S''' + S^{v} - S^{v''}, \&c.} \times \frac{1}{r^{n-1}}$.

And the co-sine of $\overline{\alpha+\beta+\gamma+\delta, \&c.} = S\overline{- S'' + S^{iv} - S^{v}, \&c.} \times \frac{1}{r^{n-1}}$.

XV. Also putting T' for the sum of the tangents of the arcs, α, β, γ, &c. T'', T''', T^{iv}, &c. for the sum of the products of every two, three, four, &c. tangents; and

$$\begin{aligned} A &= T' \\ B &= AT'' - T''' \\ C &= BT'' - AT^{iv} + T^{v} \\ D &= CT'' - BT^{iv} + AT^{vi} - T^{vii} \\ E &= DT'' - CT^{iv} + BT^{vi} - AT^{viii} + T^{ix}. \qquad \text{Put } R = \frac{1}{rr} \\ &\&c. \end{aligned}$$

Then the tangent of $\overline{\alpha+\beta+\gamma+\delta, \&c.} = A + BR + CR^{2} + DR^{3} + ER^{4}$; &c.

XVI. Hence, the sine, tangent, and secant, of any arc a, being represented by s, t, f, the co-sine, co-tangent, and co-secant, by s', t', f'; those of the arc na are expressed as in the following theorems.

Putting $n' = n.\frac{n-1}{2}$; $n'' = n'.\frac{n-2}{3}$; $n''' = n''.\frac{n-3}{4}$; $n^{iv} = n'''.\frac{n-4}{5}$; &c.

Sine of $na = \overline{nA - n''AP + n^{iv}BP - n^{vi}CP + n^{viii}DP, \&c.} \times \frac{s'^{n-1}}{r^{n-1}}$;

where $P = \frac{ss}{s's'}$; $A = s$; $B = AP$; $C = BP$; $D = CP$; &c.

Or $= \overline{ns - \frac{n-1}{2}.\frac{n-2}{3}AP + \frac{n-3}{4}.\frac{n-4}{5}BP - \frac{n-5}{6}.\frac{n-6}{7}CP \&c.} \times \frac{s'^{n-1}}{r^{n-1}}$;

where A, B, C, &c. stand for the respective preceding terms.

Or $= ns + \frac{1+n}{2}.\frac{1-n}{3}AQ + \frac{3+n}{4}.\frac{3-n}{5}BQ + \frac{5+n}{6}.\frac{5-n}{7}CQ + \frac{7+n}{8}\frac{7-n}{9}DQ$, &c.

where $Q = \frac{ss}{rr}$; A, B, C, &c. stand as before.

XVII. Co-sine of $na = \overline{1 - n^{\prime}P + n^{\prime\prime\prime}P^2 - n^{v}P^3 + n^{v\prime}P^4, \&c.} \times \frac{s^{\prime n}}{r^{n-1}}$, where $P = \frac{tt}{ss}$

$$\text{Or} = r + \frac{0+n}{1}\cdot\frac{0-n}{2}AQ + \frac{2+n}{3}\cdot\frac{2-n}{4}BQ + \frac{4+n}{5}\cdot\frac{4-n}{6}CQ + \frac{6+n}{7}\cdot\frac{6-n}{8}DQ, \&c.$$

where $Q = \frac{ss}{r}$; and A, B, C, &c. stand for the respective preceding terms.

Or put $M = \overline{\frac{2s}{r}}\Big|^{n} \times r$; $N = \frac{rr}{4s^{\prime}s^{\prime}}$; $A = \frac{1}{2}$; $B = AN$; $C = BN$; $D = CN$, &c; $p = n$; $p^{\prime} = n-1$; $p^{\prime\prime} = n-2$, &c.

And $a^{\prime} = p$; $b^{\prime} = p.\frac{1}{2}p^{\prime\prime}$; $c^{\prime} = p\,\frac{1}{2}p^{\prime v}.\frac{1}{3}p^{v}$; $d^{\prime} = p\,\frac{1}{2}p^{v}.\frac{1}{3}p^{v\prime}.\frac{1}{4}p^{v\prime\prime}$; $e^{\prime} = p.\frac{1}{2}p^{v\prime}.\frac{1}{3}p^{v\prime\prime}.\frac{1}{4}p^{v\prime\prime\prime}.p\frac{1}{5}p^{\prime x}$; &c.

The co-sine of $na = \overline{A - Ba^{\prime} + Cb^{\prime} - Dc^{\prime} + Ed^{\prime}, \&c.} \times M.$

XVIII. Let $A = -n^{\prime\prime} + nn^{\prime}$
$B = +n^{\prime v} - nn^{\prime\prime\prime} + An^{\prime}$
$C = -n^{v\prime} + nn^{v} + Bn^{\prime} - An^{\prime\prime\prime}$
$D = +n^{v\prime\prime\prime} - nn^{v\prime\prime} + Cn^{\prime} - Bn^{\prime\prime\prime} + An^{v}$
&c.

$A = \frac{1}{n}.n^{\prime\prime} - n^{\prime}$
$B^{\prime} = \frac{1}{n}\,\overline{n^{\prime\prime}A^{\prime} - n^{\prime v}} + n^{\prime\prime\prime}$
$C^{\prime} = \frac{1}{n}.\overline{n^{\prime\prime}B^{\prime} - n^{\prime v}A^{\prime} + n^{v\prime}} - n^{v}$
$D^{\prime} = \frac{1}{n}.\overline{n^{\prime\prime}C^{\prime} - n^{\prime v}B^{\prime} + n^{v\prime}A^{\prime} - n^{v\prime\prime\prime}} + n^{v\prime\prime}$
&c.

The tangent of $na = nt + At^3r^{-2} + Bt^5r^{-4} + Ct^7r^{-6} + Dt^9r^{-8}$ &c.

Or $= \overline{n + AN + BN^2 + CN^3 + DN^4 + \&c.} \times t$, where $N = \frac{tt}{rr}$

Or $= na^{\prime} + Ab^{\prime} + Bc^{\prime} + Cd^{\prime} + De^{\prime}$, &c. where $a^{\prime} = t$; $b^{\prime} = Na^{\prime}$; $c^{\prime} = Nb^{\prime}$; $d^{\prime} = Nc^{\prime}$; &c.

Or $= \dfrac{n - n^{\prime\prime}N + n^{\prime v}N^2 - n^{v\prime}N^3 + n^{v\prime\prime\prime}N^4, \&c.}{1 - n^{\prime}N + n^{\prime\prime\prime}N^2 - n^{v}N^3 + n^{v\prime\prime}N^4, \&c.} \times t.$

Co-tang^t of $na = \overline{r^2 + A^{\prime}t^2 + B^{\prime}t^2N + C^{\prime}t^2N^2 + D^{\prime}t^2N^3 + E^{\prime}t^2N^4 \&c.} \times \frac{rr}{nt}$; where $N = \frac{tt}{rr}$

Or $= \overline{1 + A^{\prime}N + B^{\prime}N^2 + C^{\prime}N^3 + D^{\prime}N^4 + E^{\prime}N^5, \&c.} \times \frac{1}{n}r^2t$; where $N = \frac{rr}{t^{\prime}t^{\prime}}$

Or $= \dfrac{1 - n^{\prime}N + n^{\prime\prime\prime}N^2 - n^{v}N^3 + n^{v\prime\prime}N^4 - n^{\prime x}N^5, \&c.}{n - n^{\prime\prime}N + n^{\prime v}N^2 - n^{v\prime}N^3 + n^{v\prime\prime\prime}N^4 - n^{x}N^5, \&c.} \times \frac{rr}{t}$; where $N = \frac{tt}{rr}$

XIX. Let $A = n^{\prime}$
$B = An^{\prime} - n^{\prime\prime}$
$C = Bn^{\prime} - An^{\prime\prime\prime} + n^{v}$
$D = Cn^{\prime} - Bn^{\prime\prime\prime} + An^{v} - n^{v\prime\prime}$
&c.

$A^{\prime} = \frac{1}{n}.n^{\prime\prime}$
$B^{\prime} = \frac{1}{n}.\overline{n^{\prime\prime}A^{\prime} - n^{\prime v}}$
$C^{\prime} = \frac{1}{n}.\overline{n^{\prime\prime}B^{\prime} - n^{\prime v}A + n^{v\prime}}$
$D^{\prime} = \frac{1}{n}.\overline{n^{\prime\prime}C^{\prime} - n^{\prime v}B^{\prime} + n^{v\prime}A^{\prime} - n^{v\prime\prime\prime}}$
&c.

Secant of $na = \overline{1 + AN + BN^2 + CN^3 + DN^4 + EN^5, \&c.} \times M.$

Or $= \dfrac{1}{1 - n^{\prime}N + n^{\prime\prime\prime}N^2 - n^{v}N^3 + n^{v\prime\prime}N^4, \&c.} \times M$; where $N = \frac{tt}{rr}$, $M = \frac{rs^{n}}{r^{n}}$

Co-secant of $na = \overline{1 + A^{\prime}N + B^{\prime}N^2 + C^{\prime}N^3 + D^{\prime}N^4 + E^{\prime}N^5, \&c.} \times M$; where $N = \frac{tt}{rr}$, $M = \frac{rrs^{n}}{ntr^{n}}$

Or $= \dfrac{1}{n - n^{\prime}N + n^{\prime\prime}N^2 - n^{v\prime}N^3 + n^{v\prime\prime\prime}N, \&c.} \times M$; where $N = \frac{tt}{rr}$, $M = \frac{r^{n-2}}{ts^{n-2}}$

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XX. Let c be the chord of an arc (a) of the circumference of a circle, whose diameter is d. Put $N = \frac{cc}{dd}$.

The chord of $na = nc + \frac{1+n}{2} \cdot \frac{1-n}{3} AN + \frac{3+n}{4} \cdot \frac{3-n}{5} BN + \frac{5+n}{6} \cdot \frac{5-n}{7} CN + \frac{7+n}{8} \cdot \frac{7-n}{9} DN$, &c. where A, B, C, &c. stand for the respective preceding terms.

As the preceding theorems are easily deduced from the first, so the following are most readily seen to be the immediate consequences of these; and all depending upon no other principles than what are generally made use of in common computations.

XXI. Putting s, s', t, t', $\int$, $\int'$, for the sine, co-sine, tangent, co-tangent, secant, co-secant, of an arc (a), and v its versed sine; let $q' = \frac{1}{2}$; $q'' = \frac{1}{3}q'$; $q''' = \frac{1}{4}q''$; $q^{iv} = \frac{1}{5}q'''$; $q^{v} = \frac{1}{6}q^{iv}$; &c. $N = \frac{aa}{rr}$.

Then $s = \overline{1 - q''N + q^{iv}N^2 - q^{vi}N^3 + q^{viii}N^4 + q^{x}N^5, \&c.} \times a$.

$= a - q''a^3r^{-2} + q^{iv}a^5r^{-4} - q^{vi}a^7r^{-6} + q^{viii}a^9r^{-8}$, &c.

$= a - \frac{1}{2.3} AN + \frac{1}{4.5} BN - \frac{1}{6.7} CN + \frac{1}{8.9} DN$, &c. where A, B, C, &c. stand for the respective preceding terms.

And $s' = r - q'a^2r^{-1} + q'''a^4r^{-3} - q^{v}a^6r^{-5} + q^{vii}a^8r^{-7}$, &c.

$= \overline{1 - q'N + q'''N^2 - q^{v}N^3 + q^{vii}N^4 \quad q^{x}N^5, \&c.} \times r$.

$= r - \frac{1}{1.2} a^2r^{-1} + \frac{1}{3.4} AN - \frac{1}{5.6} BN + \frac{1}{7.8} CN$, &c. A, B, C, &c. as before.

XXII. Also $v = q'a^2r^{-1} - q^{iv}a^4r^{-3} + q^{vi}a^6r^{-5} - q^{viii}a^8r^{-7}$, &c.

$= \frac{1}{1.2} a^2r^{-1} - \frac{1}{3.4} AN - \frac{1}{5.6} BN - \frac{1}{7.8} CN - \frac{1}{9.10} DN$, &c.

$= \overline{\frac{1}{1.2} N - \frac{1}{3.4} AN - \frac{1}{5.6} BN - \frac{1}{7.8} CN, \&c.} \times r$. A, B, C, &c. as before.

XXIII. Let $A = +q' - q''$ And $A' = -A$

$B = -q''' + q^{iv} + Aq'$ $B' = -B - AA'$

$C = +q^{v} - q^{vi} + Bq' - Aq'''$ $C' = -C - BA' - AB'$

$D = -q^{vii} + q^{viii} + Cq' - Bq''' + Aq^{v}$. $D' = -D - CA' - BB' - AC'$.

&c. &c.

Tangent $t = a + Aa^3r^{-2} + Ba^5r^{-4} + Ca^7r^{-6} + Da^9r^{-8}$, &c.

Or $= \overline{1 + AN + BN^2 + CN^3 + DN^4 + EN^5, \&c.} \times a$.

Co-tangent $t' = a^{-1}r^2 + A'a + B'a^3r^{-2} + C'a^5r^{-4} + D'a^7r^{-6}$, &c.

Or $= \overline{rr + A'a^2 + B'Na^2 + C'N^2a^2 + D'N^3a^2, \&c.} \times \frac{1}{a}$.

XXIV.

XXIV. Also let $\alpha = +q'$ And $\alpha' = +q''$

$\beta = -q''' + \alpha q'$ $\beta' = -q^{iv} + \alpha' q''$

$\gamma = +q^{v} - \alpha q''' + \beta q'$ $\gamma' = +q^{vi} - \alpha' q^{iv} + \beta' q''$

$\delta = -q^{vii} + \alpha q^{v} - \beta q''' + \gamma q'$. $\delta' = -q^{viii} + \alpha' q^{vi} - \beta' q^{iv} + \gamma' q''$.

&c. &c.

Secant $s = r + \alpha a^2 r^{-1} + \beta a^4 r^{-3} + \gamma a^6 r^{-5} + \delta a^8 r^{-7}$, &c.

Or $= \overline{1 + \alpha N + \beta N^2 + \gamma N^3 + \delta N^4, \text{&c.}} \times r$.

Co-secant $\grave{s} = a^{-1} r^2 + \alpha' a + \beta' a^3 r^{-2} + \gamma' a^5 r^{-4} + \delta' a^7 r^{-6}$, &c.

Or $= \overline{rr + \alpha' aa + \beta' Naa + \gamma' N^2 aa + \delta' N^3 aa, \text{&c.}} \times \frac{1}{a}$. where $N = \frac{aa}{rr}$.

XXV. Putting $p' = \frac{1}{2}$; $p'' = \frac{3}{4}p$; $p''' = \frac{5}{6}p''$; $p^{iv} = \frac{7}{8}p'''$; $p^{v} = \frac{9}{10}p^{iv}$; &c. $N = \frac{ss}{rr}$

Then arc $a = \overline{1 + \frac{1}{3}p'N + \frac{1}{5}p''N^2 + \frac{1}{7}p'''N^3 + \frac{1}{9}p^{iv}N^4, \text{&c.}} \times s$.

Or $= s + \frac{1}{3}p'AN + \frac{1}{5}p''BN + \frac{1}{7}p'''CN + \frac{1}{9}p^{iv}DN$, &c.

Or $= s + \frac{1.1}{2.3}AN + \frac{3.3}{4.5}BN + \frac{5.5}{6.7}CN + \frac{7.7}{8.9}DN$, &c. where A, B, C, &c. stand for the respective preceding terms.

XXVI. If v is the versed sine of an arc a, diameter being d, $M = \frac{v}{d}$, $R = \sqrt{dv}$.

Then arc $a = \overline{1 + \frac{1.1}{2.3}M + \frac{3.3}{4.5}AM + \frac{5.5}{6.7}BM + \frac{7.7}{8.9}CM, \text{&c.}} \times R$; A, B, C, &c. are as before.

XXVII. And putting $N = \frac{tt}{rr}$, $A = t$, $B = AN$, $C = BN$, $D = CN$, &c.

Then arc $a = t - \frac{1}{3}AN + \frac{1}{5}BN - \frac{1}{7}CN + \frac{1}{9}DN + \frac{1}{11}EN$, &c.

Or $= \overline{1 - \frac{1}{3}N + \frac{1}{5}N^2 - \frac{1}{7}N^3 + \frac{1}{9}N^4 + \frac{1}{11}N^5, \text{&c.}} \times t$.

XXVIII. Also, if c is the chord of an arc (a); and $N = \frac{cc}{dd}$,

Then arc $a = c + \frac{1.1}{2.3}AN + \frac{3.3}{4.5}BN + \frac{5.5}{6.7}CN + \frac{7.7}{8.9}DN$, &c. where A, B, C, &c. stand for the respective preceding terms.

ADVERTISEMENT.

THE Reader is desired to correct the Title of the XII[th] Paper of the last Number of these *Transactions* in the following manner:

A Letter from Richard Brocklesby M. D. *and* F.R.S. *to the* President, *concerning the* Indian Poison, *sent over by* Don Antonio De Ullöa *of Seville*, F. R. S. *and mentioned by* M. de la Condamine, *of the* Royal Academy *of* Sciences *at* Paris, *in his late Account of the* River *of the* Amazons *in* South America.

He is also desired in the same Paper, to read towards the Bottom of the first Page, instead of *Mr. Juan Antonio de Loa*, *Don Antonio de Ulloa*. This Gentleman was one of those sent by the King of *Spain* to attend and assist the *French* Astronomers of the *Royal Academy* of *Sciences*, in their late Measure of a *Degree* of *Latitude* near the *Equator*. He was taken Prisoner at *Cape Breton* in his Return home, and brought into *England*, where his Papers all relating to the Measure of the Degree, and other Astronomical and Philosophical Observations, were by the Favour of the Lords Commissioners of the Admiralty restored to him, to be published in his own Country. An Abstract of the same was however by their Lordship's Leave communicated to the *Royal Society*, by their *President*, who was entrusted with the Perusal of the same:. And the Author himself, who is a Gentleman of great Merit, was soon after unanimously chosen a Fellow of the *Society*. Some time after his Return he procured the above-mentioned Specimen of this *Indian* Poison, which he sent over together, with some Books, as a Present to his Friend the *President* of the *Society*.

ERRATA.

In *page* 456, *l.* 21. *r.* TAB. II. *Fig.* 8.

In *page* 548, *l.* 28. *add*, by the Direction of the Strokes according to the Rules of Heraldry.

In *page* 549, *l.* 10. *add*, see TAB. IX.

Printed for C. DAVIS, over-against *Gray's Inn Gate* in *Holbourn*, PRINTER to the ROYAL SOCIETY, M.DCC.XLVII.

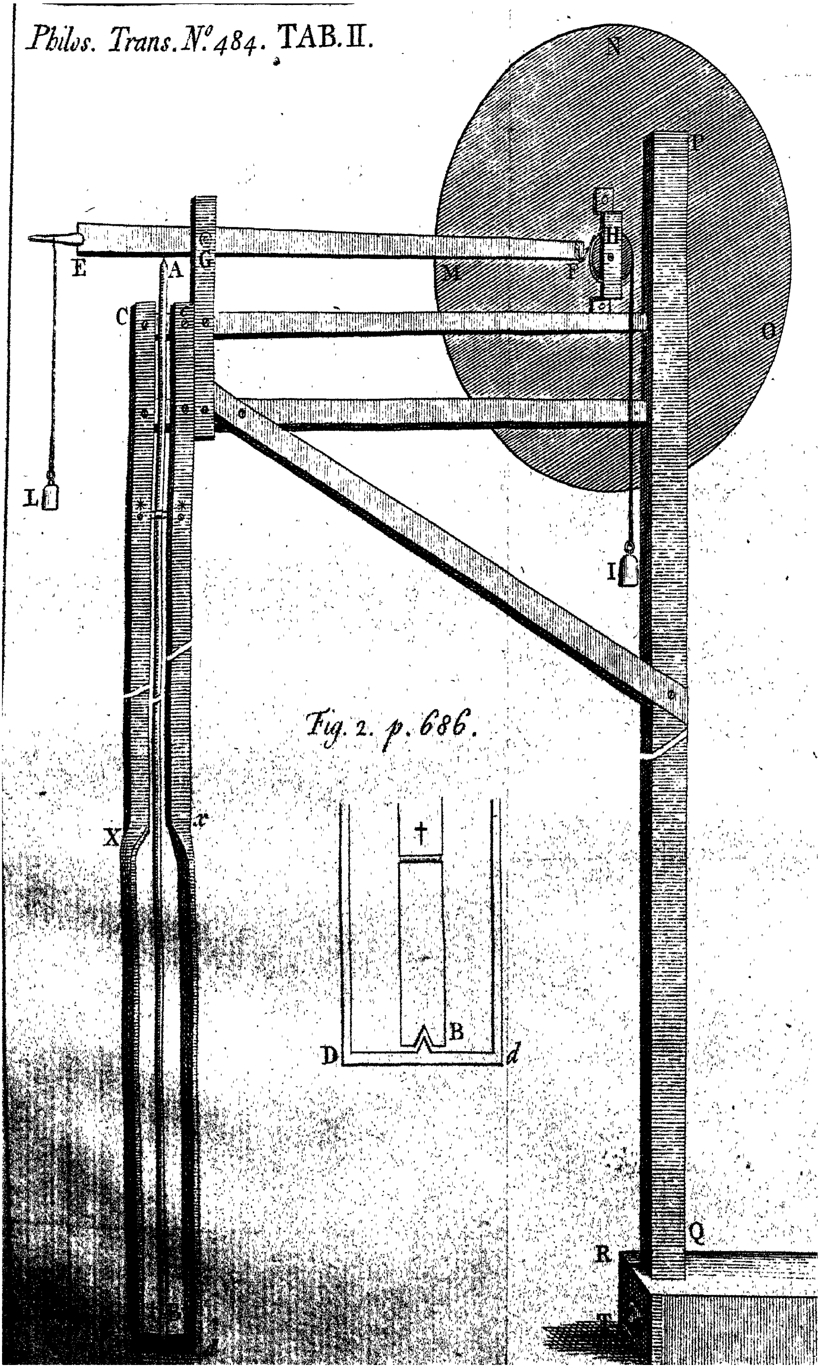
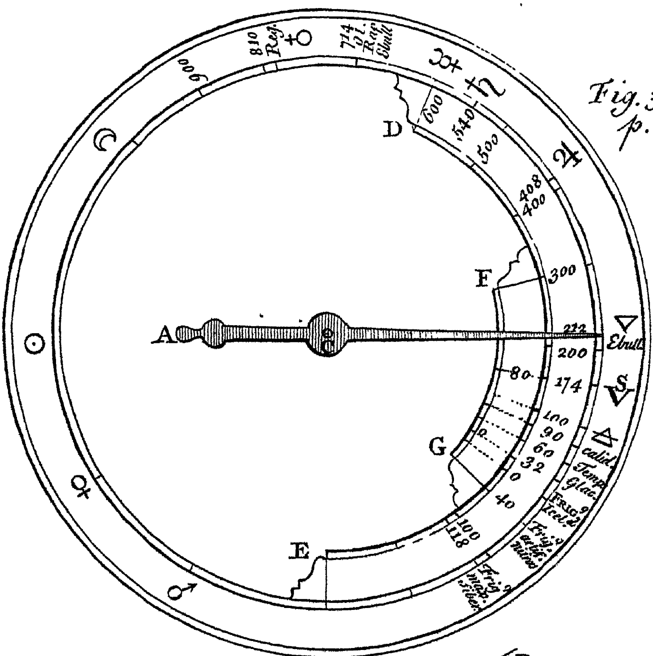


Fig. 2. p. 686.

Fig. 3. p. 688.



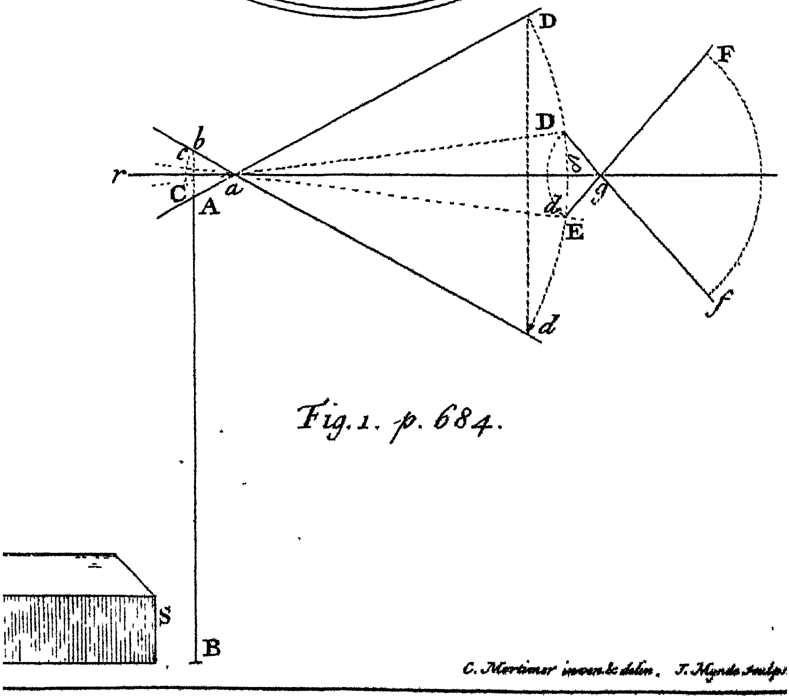


Fig. 1. p. 684.

C. Mortimer invent. & delin. J. Mynde sculp.

Philos Trans N° 484. TAB I.

Fig 1 p 575

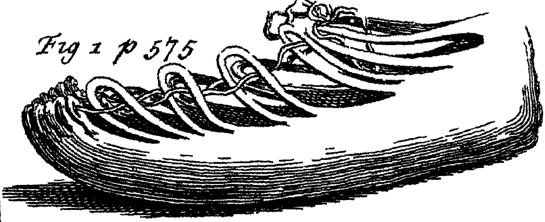
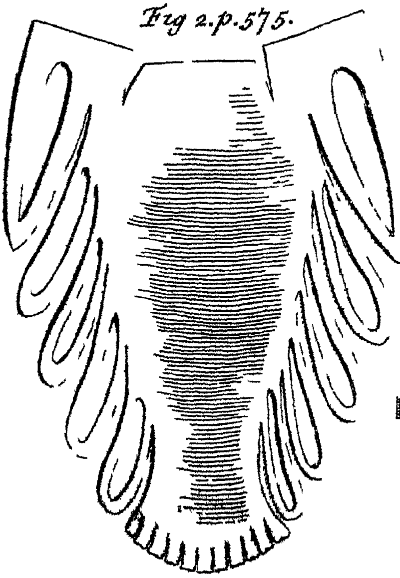


Fig 2. p. 575.



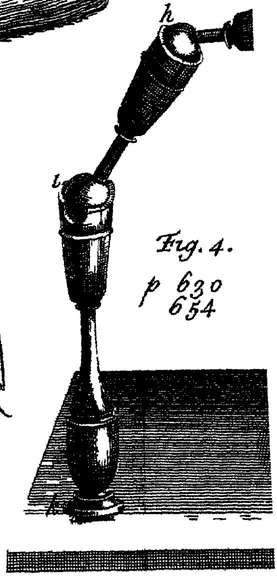


Fig. 4.
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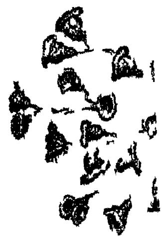
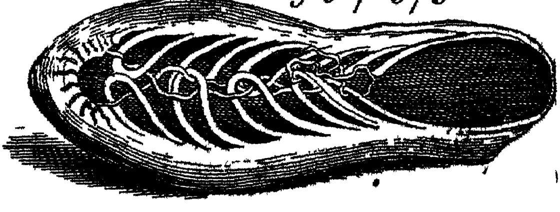


Fig 3. p. 575.



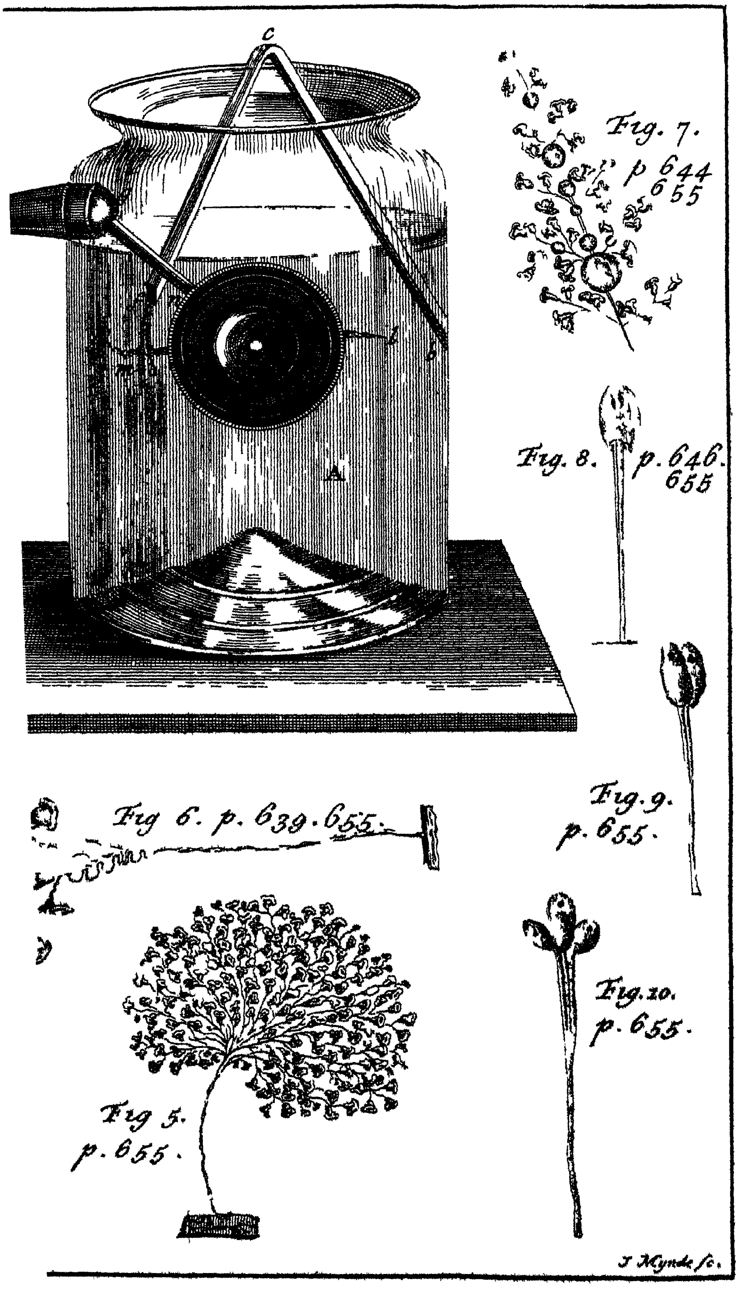

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Fig. 10.
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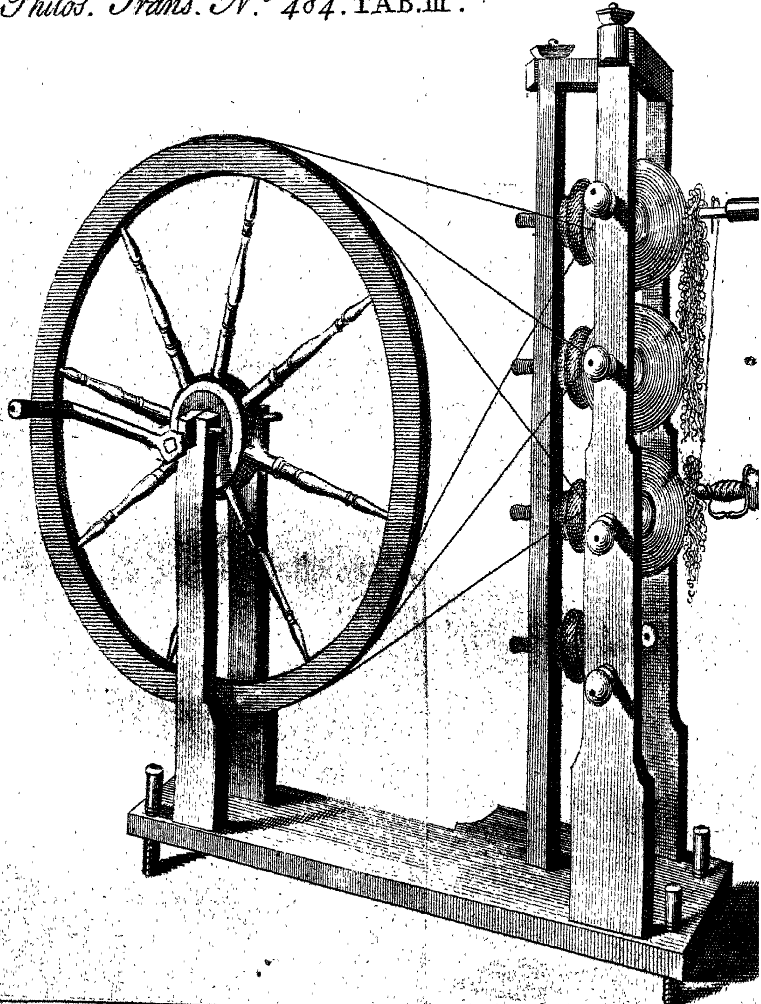
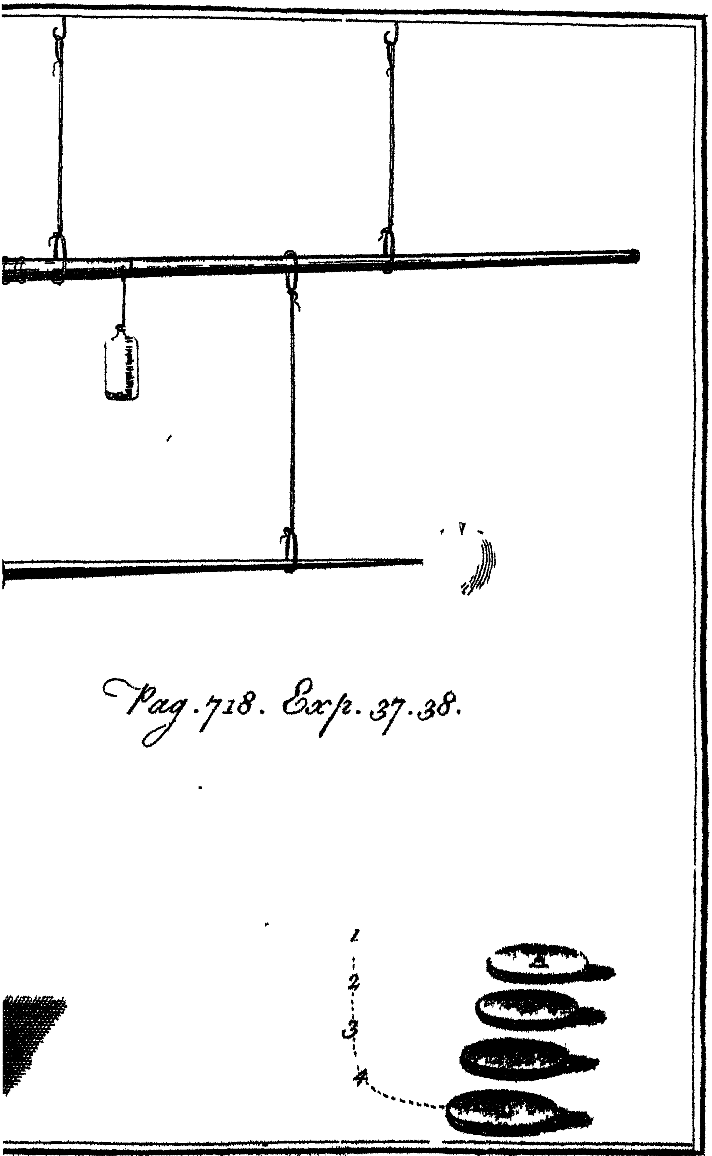
Fig 5.
p. 655.
J. Mynde sc.

Philos. Trans. N.° 484. TAB. III.





Pag. 718. Exp. 37. 38.

J. Mynde Sculp

Numb. 484.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *Octob. Nov.* and *Decemb.* 1747.

The CONTENTS.

The CONTENTS.

I. *Extract of a Letter dated at* Rome, Aug. 5. 1747. *from Mr.* Hoare, *a young Statuary, now pursuing his Studies there, to his Brother Mr.* Hoare, *an eminent Painter at* Bath, *giving a short Account of some of the principal antique* Pictures *found in the Ruins of* Herculaneum *at* Portici, *near* Naples: *Communicated by the Rev. Mr.* Birch.

Read at a Meeting of the Royal Society, October 22. 1747.

THIS City was overthrown and swallow'd up by an Earthquake near 1700 Years since.

Some of the most remarkable Curiosities, we saw, were,

1. A Picture of about five Feet long, and four Feet wide, representing the Education of *Achilles*, by his Master *Chiron* the Centaur. The Figures are about half as big as the Life. That of *Achilles* is standing in a noble Action, and is seen in Front, as the principal Object of the Picture. He seems to hearken with great Attention to, and is looking steadily on the Centaur, who is seen almost in a Side View. The Figures are both finely colour'd, and well drawn; but that of the young Man most exquisitely so.

2. Next to this is a Picture of about three Feet and an half high, and narrow, in which is a Woman sacrificing. The Figure is about two Feet high. This Picture seems to have been taken out of some Compartiment of Ornaments.

3. Next to this is a broken Piece, representing the Judgment of *Paris*. The Figures are about the same Size as that last above-mention'd. They are not intire; the bottom Part being broken off about the Knees. This is also a very fine Picture; but it is impossible to judge of all its Beauties, as it is extremely changed and decayed; which is quite contrary to all the others, but, in particular, to that of *Chiron* and *Achilles*; which is in a manner as fresh as if it had been painted but Yesterday.

4. The next is a fine Picture of the Story of *Virginia*. The Figures are something bigger than those in the abovementioned. The Characters and Expressions of the Heads are admirable. That of *Appius* gives a just Idea of the furious Transports in which the Artist designed to describe him. *Virginia* is weeping; and, in a word, all the Figures are finely disposed, and the Characters well adapted to the Subject.

5. Two large Pictures were in a Nich in a *Basilica*, about five or six Feet high. The first represents *Theseus* victorious over the *Minotaur*. He is standing in a free and fine Posture: One Foot is on the Head of the *Minotaur*. But what seemed odd to me, was the Figure of that Monster itself, which I had always seen differently represented; for, in this Picture, the Head only represents that of a Bull, which is joined to the Body of a Man. Several little *Genii*, or *Cupids* (as we call them), all seem impatient to shew their Respect to their Deliverer: One kisses his Hand, another clasps round his Leg, and several others are in different Attitudes of Gratitude.

titude. The Figures are almost as large as small Life.

6. The other Picture represents *Hercules* and the Goddess of Nature. The Figure of *Hercules* is standing (seen in a Side View) reposing on his Club; something like the Statue in the *Farnese* Palace at *Rome*. There is a Victory crowning the Hero, and the Goddess is sitting before him, and seems to applaud and thank him for his Labours. There are Numbers of symbolical Figures besides in this Picture. Behind the Goddess is a Satyr, and at *Hercules*'s Feet a Boy sucking a Doe. My Friend, who conducted me, took particular Notice, how delicately the Doe seemed to dispose of her Legs, not to hurt the Child; whilst at the same time she is licking his Knees, as a Mark of her Tenderness for him. This Picture is equal to the first-mentioned; being exquisitely finely drawn and coloured, and well composed.

7. There is a little Picture, which I thought extremely odd for its Composition. It is about 1 Foot and an half long, and eight or nine Inches high: It is a Parrot drawing a Chariot something like our modern Chaises. In the Chariot sits a sort of large Horse-fly, whose two Horns serve for the Bridle and Reins to guide the Parrot.

8. Two Pictures, of about four Feet and an half long, represent the Stage of a Theatre, with Comedians playing their Parts upon it. The Perspective in these Pictures is very well observed.

9. A Wedding, consisting of three Figures only. They are much in the same Taste of those of *Aldobrandini*'s Marriage at *Rome*. There are besides

Numbers of little Frizes reprefenting Sacrifices, and other Ceremonies, of the ancient Pagans; moft of them on black or red Grounds.

Thefe Pictures fhew, that the Antients underftood Perfpective and Landfchape, I mean, the Keeping particularly, which I have heard ftrongly difputed; but no one that has feen thefe Pictures will, I believe, make any Doubt of it.

It would be impoffible for me to give you an exact Defcription of all the Pictures; as there are fo many entire, befides the Bits, and Fragments of others. Of fome, the Heads only remain; and of others, Pieces of Figures; Numbers of fmall Landfchapes; Views of Architecture; Flowers and Fruit, painted extremely light and elegantly. There are even fome grotefque Pictures, fomething in the Tafte of *India* Painting. Moft of the fmall ones have been taken out of Compartiments: The Guardian fhewed me feveral Places from whence they had been taken. They ftill preferve a Beauty fuperior to any thing we fee now-a-days: The Colouring, Drawing, and Liberty of Pencil, may vie with the Works of any Mafter, even of *Raphael* himfelf.

There are two Rooms full of them.—— Perhaps you will fay I have given you an Account but of very few.——In the firft place I muft inform you, that no one is admitted without an Order from the King's Superintendant; and that, when one does fee them, the Guardian has Orders not to permit any Perfon to take any Sketch or Account whatever of the Pictures. My Friend went thither but

once,

once, and that after Dinner, when we came down from Mount *Vesuvius*. The Account I have herein given is all I could particularly remember in the Evening when we came home. They are continually finding more Pictures every Day; and I do assure you, that had I a Month to spare, I would willingly go on Foot to *Naples*, to have the Pleasure of studying those I have already seen, and seeing those which have been discovered since. *

N. B. Cardinal *Albani*, at *Rome*, has an antique Group of *Theseus* and the *Minotaur*; where the *Minotaur* has the Head only of a Bull, as in the Picture above-mentioned.

II. *A Letter from Mr.* G. Stovin *to his Son, concerning the Body of a* Woman, *and an* antique Shoe, *found in a Morass in the* Isle *of* Axholm *in* Lincolnshire.

Read Oct. 22. 1747.

THE Beginning of *June* last, a labouring Man, of *Amcotts* in the Isle of *Axholm*, in the County of *Lincoln*, was digging Turf or Peat in the Moors of *Amcotts*; and, at about six Foot from the Surface, his Spade cut the Toe of a Sandal, which dropped into the Pit he was graveing Peat in; also Part of the Foot dropp'd in, which terrified the Man, and he left it. Hearing of this Discovery, I went and took some Servants with me, to make further Discovery; when we soon found the other Sandal (which I now send you

* See more of these Curiosities in these *Trans.* N°. 456 and 458.

you whole and firm). It was very ſoft and pliable, and of a tawny Colour, with all the Bones of that Foot in it, and all the griſly Part of the Heel: And proceeding further, we found the Skin and Thigh-Bones, which I meaſured to be eighteen Inches long. We then found all the Skin of the lower Parts of the Body, which was of the ſame Colour of the Sandals, and very ſoft, with freſh Hair upon it, *&c.* which diſtinguiſh'd it to be a Woman, The Skin drew or ſtretch'd like a Piece of Doe-Leather, and was as ſtrong. We then found the Skin of the Arms, which was like the Top of a Muff or Glove, when the Bones were ſhaken out. We then found this Hand I have ſent, with the Nails as freſh as any Perſon's living; which are now, both Hand and Nails, ſhrunk very much, ſince it was expoſed to the Air: This Hand is the Lady's natural Skin ſo tann'd, with the Nails. We left the Bones in the Fingers, where the Nails are, for fear the Nails ſhould drop off, if that Joint was taken out.

I want to be informed what Age they wore thoſe Sandals in. Theſe muſt, I think, be very antient, and have moſt certainly been made of a raw Hide, by reaſon they, and the Skin of the Lady, were both of one Colour, and both had one Tanner; which I preſume, is the Moor-Water; which is exactly of the Colour of Coffee; and made ſo by reaſon of ſuch great Quantities of Oak and Fir-Wood, that we frequently dig out of theſe Moors; ſeveral Oak-Trees affording 1000 Pales for fencing, five Feet and an half long, and ſix to eight Inches broad; which Oak-Wood is eated as black as Jet. The Fir-Wood retains its Turpentine-Smell, and in hot Weather

(when

(when it is expoſed to the Sun) the Turpentine will drop from it. This Wood is frequently riven into Laths for the Roofs of Houſes or Floors; and what is remarkable, no Worm will touch them. The Pales mentioned above are ſold from 10 to 15 Shillings *per* hundred. We frequently find Hazle-nuts and Fir-Apples in Abundance; which I think is a plain Proof, that the Trees fell in Autumn, when the Fruits were at Maturity. I think Dr. *Shuckford* makes it plainly appear that the General Flood happen'd in Autumn.

This Lady in all Probability was overwhelmed by ſome ſtrong Eddy of Water; for ſhe lay upon one Side bended, with her Head and Feet almoſt together.

It appears by the Maps of the Country, that this has been the Rendezvous of all the Waters from the South, Weſt, and North Parts of the Kingdom; as for Inſtance, the River *Dun*, from *Doncaſter*, *Rotherham*, and *Sheffield*, which took in many more Streams; as the *Idle*, *Trent*, *Torn*, *Dare*, *Rother*, &c. &c. Then the River *Trent*, which runs South to *Gainsbrough*; then to *Torkſey*, *Newark*, *Nottingham*, *Derby*, *Burton* upon *Trent*, *Strafford*, *Trentham* in *Staffordſhire*: And takes in a vaſt Number of Rivulets: Then the *Ouſe*, which comes from near *Richmond*, and takes in the *Ure*, *Wharf*, *Biſhop's Dike*, *Aire*, *Calder*, and a great Number of Rivulets; which are all loſt in that famous Eſtuary the *Humber*.

It is alſo to be obſerved, that here is one Moraſs twenty Miles round, Part in *Hatfield-Chace*; another ten Miles round in the ſame Chace, where the

the famous *William* of *Lindham* had his Cell. In the middle of it, where his Body was found, for eight Miles round, is all a Morafs.

The Connoiffeurs will give you their Opinion in the chief thing I want to know, which is, in what Age thofe Sandals were worn, and by what Nation: For it is not like the *Scots* or *Irifh* Broges; though the *Scots*, I think, formerly inhabited but a little Way off, *to wit*, North of *Humber*. Perhaps the *Danes* may wear fuch, or the antient *Saxons*; for both thefe People muft be well acquainted with thefe Parts, as the *Danes* under *Edgar-Atheling* incamped a whole Winter in this Neighbourhood, and had a Station at *Gigansburgh*, now *Gainsbrough*, upon this River *Trent*.

At *Boxby* was a famous *Roman* Pavement, 15 Yards fquare, the *Roman* Road, *&c.*; alfo a fquare Platform at *Aldbrough*, which I take to be *Roman*, though no Difcoveries have as yet been made there; but at *Roxby* large Quantities of *Roman* Coins have been found. I am

Your ever affectionate Father,

G. Stovin.

P. S. As to this Water upon thefe Moors preferving human Bodies *, it is moft certain; *viz.* Part of a Body taken up at *Geel* by your Grandfather Mr. *Empfon* 50 or 60 Years ago, and one in the great Moor near *Thorn*, about 7 Years ago, with the Skin like tann'd Leather, the Hair,

* See thefe *Tranf.* No. 434. *p.* 413.

Hair, Teeth, and Nails quite fresh.——You will see the Sandal * is of one Piece of Leather, and a Seam at the Heel, with a Thong of the same Leather. *See* TAB. I. *Fig*. 2. and 3.

It is the Skin of the Hand that is stuff'd, which has suffer'd by the Spade. †

III.

* Mr. *Catesby* F. R. S. Author of the History of *Carolina*, *&c*. being present, said, this Shoe or Sandal was exactly like what the *Indians* in *Virginia* wear at this Day, and call *Mokasin*.

†That ingenious Artist and skilful Antiquary Mr. *Geo. Vertue* communicated to me (*C. M.*) his Sentiments concerning this Sandal in the following Words:

'When the above Letter was read at the *Society of Antiquaries*, 'there was produced a Hand of the Woman therein mentioned, 'and a Sandal or Shoe taken from one of her Feet; it being made 'of Leather, tann'd Ox-Hyde; but remarkable for being cut out 'of one flat Piece, (see *Fig*. 3.) so as to fold about the Foot and 'Heel; the Form and make being so contriv'd without under Heel-'Piece, as to be flat to tread on; the Shape, that of a Woman's 'Foot, and the Toe round-pointed. This being of an antient 'Form, the Society order'd an exact Draught to be taken of both 'that and the Hand; which Drawings are preserved amongst 'others belonging to that Society. It may be observed concerning 'the Antiquity and Use of Leather Shoes in *England*, that this 'Shoe or Sandal appears by its Form to be ancient. I conceive it 'was before *Edward* the IV.'s Time, when, by Custom, piked 'Shoes had increased in Length, that all such who wore 'them in excessive Length were to be mulcted, or have them 'cut shorter, in passing in or out of the City-Gates of *London*. 'This very likely had passed amongst the better sort of People 'about the Kingdom; for *Chaucer* in his Time mentions the Use 'of long piked Shoes, so long as to be tied up by Strings or small 'Chains to their Knees.

'Thus it might have been with Mens Shoes, but not in so long 'a Degree for Womens Use; tho' observing antient Pictures of 'Men and Women in Books of Illuminations, piked Shoes appear 'in several Reigns from *Ed*. III. to *Rich*. III. in *England*.

 'Also

III. *A Letter from Mr.* Henry Baker *F.R.S. to the* President, *concerning the* Grubbs *destroying the Grass in* Norfolk.

S I R,

Read Nov. 5. 1747.

HAVING seen some Letters lately sent from the Counties of *Norfolk* and *Suffolk*, giving an Account, that prodigious Numbers of what one Letter calls *Grubs*, and another large *Maggots*, full as thick and almost as long as a Man's little Finger, are dispersed over the Fields, and do abundance of Mischief in those Counties, I immediately imagined (tho' the Accounts were very imperfect, being sent by People wholly ignorant of Natural History), that they must be the *Aureliæ* or *Chrysalides* of some Species of Beetle: And desiring to get what farther Information I could concerning them, I wrote with that Intent to my ingenious Friend Mr. *Arderon* at *Norwich*, F.R.S. whose Answer (with some Additions of my own) I shall

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'Also on our antient Monuments of Stone or Alabaster cum-
'bent Statues have mostly piked Shoes. But some of earlier Date
'than *Ed.* III. have broad turn-up Shoes at the Toes, of the same
'like Form and Make as this Womans. The Mens broad Toes,
'and the Womens narrow,

'Therefore I conclude this very Sandal could not well be earlier
'than *Ed.* I. or *Hen.* III.; also, that the cutting the Form, and
'sowing to form the Heel cleverly, by a stitching behind the Heel
'with a small Leather Thong, may have been in Use before that
'of waxed Thread used by Shoe-makers, formerly called Cord-
'wainers.'

Fig. 1. shews the Shoe side-ways, laced, as when upon the Foot.
Fig. 2. the same seen from above.
Fig. 3. the same unlaced, and laid flat, to shew the manner of its being cut out of the raw Hide.

ſhall beg Leave to lay before you, after firſt intreating your Patience to read a Deſcription of theſe Inſects, publiſhed in the *London Evening Poſt* of *October* the 29th, as it is the ſame in Subſtance with the private Letters that put me on enquiring after them.

Extract of a Letter from Norfolk, *to a Friend in* London.

'I Forgot to tell you in my laſt of the Grubs that are in many Parts of our Country. They attack 'the Corn-Fields ſometimes, and ſpoil all the Crops, 'but haunt chiefly the richeſt Meadows, where they 'work between the Turf and the Soil, eating the 'Roots of the Graſs to that degree, that the Turf 'riſes and rolls up, with almoſt as much Eaſe as if 'it was cut with a Turfing-Spade; and underneath 'the Soil is turn'd to a ſoft Mould, like a Bed in a 'Garden, for about an Inch deep; in which lie the 'Grubs, in a curved Poſture, upon their Backs, all 'and every one of them with only the Tips of their 'two Ends in Sight, the reſt of their Bodies buried 'in the Mould. They are in general about an Inch 'and an half long, and as big as the Stem of a To-'bacco Pipe, near the Bowl; they have red Heads, 'white ſhining Bodies, a little hairy on the Back, 'and the Rump End is ready to burſt with a dirty 'looking Stuff, eaſily ſeen through the tranſparent 'Skin; they have ſix hairy Legs, three on each Side, 'all near the Head, two *Forceps*, or Jaws, like a 'Hornet, with which they cut aſunder the Roots 'of the Graſs, and deſtroy whole Meadows, with-

'out any Remedy yet found out to destroy them; 'they have no Eyes, as can be seen. Whether they 'are in their ultimate State, or are to be Flies, I 'know not, for we could find no *Aurelia*. The 'first News we heard of them was about two Years 'ago, by a Gentleman who lives near *Norwich*, 'and then were supposed to be new Comers. This 'Summer they have been much in *High Suffolk*, 'to the Farmers and Graziers great Loss; and we 'now hear they are likewise in some Part of *Essex*; 'they are often discover'd by Hogs, who, as I am 'inform'd, are greedy of them at first, but having 'once had their Bellies full, never care for them 'after.'

Of these Grubs Mr. *Arderon* gives the following Information.—— They are, says he, a Species of Insects but too common about *Norwich*, and, to my own Knowledge, have been more or less numerous in this County for these twenty Years past. They are the *Erucæ* of the *Scarabæus arboreus vulgaris major* of Mr. *Ray*, that is the Tree-Beetle, or blind Beetle, vulgarly in *Norfolk* called the *Dor*.

In different Parts of *England* they are called the Brown Tree Beetle, the Blind Beetle, the Chafer, the Cock-Chafer, the Jack-Horner, the Jeffry-Cock, the May-Bug, and the Dor. By the *Dutch* they are named *Baum-kaefer*, *Roub-kaefer*, *Koren Worm*, or Corn-Worm, because they destroy the Roots of Corn; and in *Zealand*, *Molenaers* or *Millers*, as *Goedartius* says, *Chap.* 78. because they bite the Leaves of several Sorts of Trees into Particles as small as if they were ground. In *England* I have likewise heard them called

called *Millers*; but ſuppoſed it to be from a white mealy Powder wherewith their Wings are covered. The *French* call them *Hanetons*.

This Inſect has two Pair of Wings, one filmy, and the other ſcaly: The firſt Pair fold together under the latter, and remain quite hid, unleſs when ſpread out for Flight. The *Elytra* or caſe Wings are of a reddiſh light-brown Colour, and ſeem ſprinkled with a white Powder that may eaſily be wiped off; the Legs and pointed Tail are whitiſh, the reſt of the Body brown, except at each Joint on the Sides of the Belly, where there is an indented Line of white. The Circles round the Eyes are yellowiſh, and ſo are the *Antennæ*, which are ſhort, and terminated by lamellated ſpreading Tufts, capable of being opened more or leſs.

It is probable the Females make Holes in the Ground with their ſharp Tails, and there depoſit their Young: But whether at firſt they are ſmall *Erucæ*, or Eggs from whence ſuch *Erucæ* are hatched, I don't pretend to ſay: 'Tis certain however, that theſe *Erucæ* are extremely miſchievous, by devouring the Roots of almoſt every thing where they come, and in ſome Grounds they are to be found in ſuch Numbers as is ſcarcely credible.

I have ſeen, ſays Mr. *Arderon*, whole Cloſes of fine flouriſhing Graſs, in Summer-time, become wither'd, dry, and as brittle as Hay in a few Weeks, by this Vermin's eating off the Roots; in doing which they are ſo dextrous, that many Yards of this wither'd Graſs might be rolled up in one Piece, all the Fibres that faſten'd it to the Ground being gnaw'd away.

Cloſes

Closes of Turnips often undergo the same Fate from these devouring Insects, which one would think designing to do as much Mischief as possible (could we suppose them capable of any Design); for when one of them fixes upon a Turnip, he eats only the middle small Root, which soon causes it to wither and die, and then moves on to the next. In like manner they destroy the Roots of Wheat, Rye, *&c.* and almost every other useful Vegetable that happens in their Way. And what makes this Pest the more deplorable, is the long Time of their Continuance in their *Eruca*, or most mischievous State, which, according to *Goedartius*, is four Years at least: But *Mouffet* writes, that in *Normandy* they are observed to be most numerous every third Year, which is therefore called *L' An des Hannetons*. And it is not improbable, that in the open Fields where they are well fed, they may come to their perfect State a Year sooner than those did which *Goedartius* almost starved in glass Jars.

Mr. *Arderon* says, he has frequently been told by People of Credit and Observation, that neither the severest Frosts of our Climate, nor the being immerged in Water, will destroy these *Erucæ*; some having been exposed for many Days to the keenest Frosts, and others cover'd with Water for as long a Time, which notwithstanding were found to revive, and become as vigorous as ever.

Crows and Hogs devour these *Erucæ* greedily; but their Numbers are too great to be much diminished thereby. The most effectual Way, tho' very laborious, is to beat them off the Trees in the Daytime with long Poles, and then sweep them together

ther and burn them. On a Farm at *Heathal*, near 5 Miles S.W. from *Norwich*, of 80 *l. per Annum*, belonging to St. *Helen*'s Hoſpital in this City, in the Occupation of Mr. *James Ebdin*, theſe Inſects were ſo numerous laſt Year, that the Farmer and his Servants affirmed they gather'd eighty Buſhels of them, the *Erucæ* of which had ſo ſpoiled the Produce of his Farm, that the Court of this City, in Compaſſion of the poor Man's Misfortune, allowed him 25 *l.* The Order for which I ſend you a Copy of, dated *Dec.* 5. 1746.

In the Day-time few of the Beetles fly about, but conceal themſelves under the Leaves of Oaks, Sycamores, Limes, *&c.* where they ſeem aſleep till near Sun-ſet, when they take Wing and fly about the Hedges, as thick as Swarms of Bees; at which time they frequently daſh themſelves againſt People's Faces with great Violence, and by their ſo doing occaſioned the common Proverb, *As blind as a Beetle.*

Mouffet tells us, it is recorded, that on the 24th of *February*, in the Year 1574. there fell ſuch a Multitude of theſe Inſects into the River *Severn*, that they ſtopp'd and clogg'd the Wheels of the Water-Mills: As to which I muſt take notice, that their coming ſo early in the Year was no leſs extraordinary than their Multitudes; for the larger Species ſeldom appear till the Month of *May*; and a ſmaller Sort, which come out in *July* and *Auguſt*, are ſeldom ſeen after the Evenings grow cold.

We are told in the *Tranſactions* of the *Dublin* Society, that the Country People in one Part of that Kingdom ſuffer'd ſo greatly by the Devaſtation made by

by these Insects, that they set Fire to a Wood some Miles in Length, which parted two adjacent Counties, to prevent their dispersing themselves any farther that Way.

This, Sir, is all I shall trouble you with at present concerning the Grubs mentioned in the News-Paper, as well as in private Letters, without distinguishing of what Kind they are; and if this Information may prove acceptable, it will be a singular Pleasure to him who begs the Honour to be esteemed,

Strand, Nov. 5. 1747.

SIR,
Your most obedient,
and most humble Servant,

H. Baker.

Mr. *Arderon*, in a subsequent Letter to Mr. *Baker*, writes, that Mr. *Ebdin* solemnly declared, before the Committee of St. *Helen's* Hospital, that the Damage done to him that Year amounted to 100 *l.* and upwards. Three Gentlemen, appointed to inspect into the Truth of his Complaint, visited the Farm in the Harvest-Time, when, amongst other things, they found those Ears of Wheat, which had Part of their Roots eaten off, to look pale and wan, and when rubb'd, to afford nothing but small wither'd Corn, not fit for any Use, and the Straw came up with the least Touch; and that these *Eruca's* do most Damage where the Soil is richest.

Tho' many burn these Flies, when they have beat them off the Trees, Mr. *Ebdin's* Method was to spread Cloths under the Trees, where he saw them hanging thickest, and when beaten off, to wrap them up in the Cloths, and beat them to Pieces with wooden Beetles.

Mr. *Arderon* says, he had the above Remarks from Mr. *Richard Humfry* one of the Committee who inspected the Farm; and he adds that some ingenious Persons account for the large Increase of these Insects from the Decrease of Rooks in this County, which they say greedily devour them in their Grub-State; and the Decrease of Rooks is owing to most of the ancient Rookeries having been destroyed by the large Fall of Timber-Trees made of late Years, which has obliged the Rooks to remove into other Parts of the Kingdom.

Nov. 21. Mr. *Arderon* dug up with his Cane two of these *Eruca's* within a Foot of one another in the *Upper Close*, near the middle of the City of *Norwich*, surrounded with Houses; they were about half-grown, an Inch and half long, and two Tenths in Diameter.

IV.

IV. *A Letter from* J. Wall *M. D. to* Edward Wilmot *M. D. F. R. S. and Physician to* His Majesty, *concerning the Use of the* Peruvian Bark *in the* Small Pox.

Read Nov. 5. 1747.

THERE is not perhaps any Disease more fatal than the Small-Pox, when attended with Hæmorrhages, purple Spots, and other terrible Symptoms. I think one might venture to affirm, that, in proportion to the Number of the Sick, fewer recover under these Circumstances, when treated by the common Methods, than do even from the Plague itself. A Remedy therefore in any Degree adequate to the Malignancy of the Disease, which could restore the broken Crasis of the Blood, and correct the putrid gangrenous Disposition of the Juices, must be of infinite Service to Mankind; and such a Remedy the *Bark* appears to be.

The first Author I meet with, who mentions the Use of the *Bark* in any Stage of the Small-Pox, is Dr. *Morton*; and he recommends it only in the (*a*) Decline of the Disease, when the secondary Fever is

(*a*) Ubi viribus veneni, durante exanthematum eruptione, ptyalismo vel quovis alio modo ex parte fractis, licet non penitus deletis, atque inde usque ad stadium declinationis intus cohibitis et revirescentibus, febris *recidiva indolem præbuerit benignam, periodicis exacerbationibus & remissionibus sese alternatim excipientibus* (quem typum in variolis mediis, prope ad malignas accedentibus, fere semper observare licet) curatio nulla methodo aut

is mild, and puts on the Type of an Intermittent. But as the *Primæ Viæ* are then very much loaded, even in the mildest Sorts of the confluent Small-Pox, this Remedy (if used only in that Stage) does not seem to promise much; at least, not till the *Saburra* in the first Passages be in some degree carried off. And accordingly, the learned and judicious (*b*) Dr. *Frewin* found that it did not at all answer, even under his Direction, till the first Passages had been emptied.

Since that time, the good Effects of the *Bark* in Mortifications and putrid Ulcers have been discover'd; and Dr. *Monro*, reasoning from thence, did some time ago (*c*) recommend it in the first Stages of the Small-Pox; as promoting the Maturation, and procuring a mild well-condition'd *Pus*. On his Authority alone, had I no other Reasons, I should have been induced to try this Medicine in some bad kinds of that Distemper, where the Suppuration does not proceed well; but having also seen the *Bark* of great Service in other Diseases, where the Texture of the Blood seem'd much broken; and particularly in Petechial and Purple Fevers, attended with Hæmorrhages,

aut remedio certius ac felicius absolvitur, quam celebri antidoto, cortice sc. *Peruviano*, qui relliquias veneni subigendo, ut in cæteris quibuscunque
febrilem
duum a
subsequente reconvalescit, &c. &c. *Morton Pyretolog. De Variolis*, *Cap.* 10.

(*b*) In his Letter to the late learned Dr. *Friend*. *Comment. de Febribus*, 5.

(*c*) Med. Essays; *Edinburgh*, Vol. 10. Art. 10.

rhages, and other terrible Symptoms; I was the more ready to conclude that it might be of great Use in the Small-Pox under the like Circumstances; more especially, as Dr. *Monro*, in the Paper above-mention'd, assures us, that, by its Use, '*Petechiæ*, in 'several variolous Patients, became gradually more 'pale-colour'd, and at last disappear'd.' I was the less fearful of making an Experiment which was new (at least in these Parts) in a Distemper attended with such fatal Symptoms, because scarce any recover under them by the common Methods. For, to use the late celebrated Dr. *Freind*'s Words [*Epist. de Purgant.*], '*Ad has angustias cum ventum sit,* '*nequaquam oportet dubitare, an anceps præstet dare* '*remedium, an nullum: quod, utcunque anceps, potest fortasse vitam servare, potest saltem ad dies aliquot protrahere. Sed quid si non successerit? satis est, ut quis hoc Medicinæ genus jure experiri putetur, si id vel in millesimo corpore profecisse compererit.*'

The first Person to whom I gave the *Bark* in the Small-Pox, was one Mr. *Hall*, a young Gentleman of about 24 Years of Age, who lived within two Miles of this Town. After heating himself violently at a Ball, he was seiz'd with the previous Symptoms of the Small-Pox. The third Day of his Illness he sent for me; and before that time he had been blooded, had taken a few Salts and Manna, which had given him 2 or 3 Stools, and afterwards used the *Decoct. Nitros.* The Blood, which had been taken away, appear'd highly inflamed. When I saw him, I found the Pustules extremely numerous un-

der the Skin, and his Body cover'd with purple Spots: Beside this, his Nose had bled profusely, and he had had several bloody Stools; or, to speak more properly, he had voided large Quantities of Blood from the *Anus*; for it was merely Blood without Excrement. His Pulse was extremely quick, but rather weak, and the Pain in his Loins violent.

The Case appearing so desperate, I determined to give him the *Bark*; a Scruple of which he took every two or three Hours, and the *Tinct. Rosar. acidulat.* for common Drink. Within 48 Hours the purple Spots disappear'd, and the Hæmorrhages stopt intirely, his Pulse grew fuller and slower, and the Pustules came on well.

From this Time to the 9th Day after the Eruption, the Appearance of the Disease continued well in every Particular; and that whole Time he perseve'd in the Use of the *Bark*, &c. and for two or three Nights had taken a little *Syr. e Mecon.* On the 9th Day in the Morning I saw him well, considering the Disease: His Spirits were good, and he spit well, but very largely. About the middle of the Day he dropt asleep, when his Nurse left the Room, and carelesly stay'd away a considerable Time (as I remember, whilst she eat her own Dinner). At her Return, he was found with his Head dropt from off the Pillow, and dead, suffocated, probably, by the Quantity of Phlegm, which, his Head lying accidentally low, he could not discharge without Assistance.

Though in this first Instance my Patient unhappily miscarried, I thought I had sufficient Reason to be pleased with the Effects of this Medicine.

Soon after this I was called to a Son of Mr. *Yates*, a noted Farmer at *Hampton-Lovat* in this County. He was about twelve Years of Age, and this was the ſixth Day from the firſt Seizure. The Puſtules were confluent, and appear'd like bloody Pimples: His Breaſt was cover'd with *Petechiæ* and purple Spots: He had a great Itching of the Noſe, ſo that an Hæmorrhage from thence was to be fear'd. He had been delirious for two or three Days paſt, and his Pulſe was very weak, quick, and trembling. I order'd him *Extr. Cort. Peruv.* 1 Scruple every third Hour, and acidulated all his Liquors with *Ol. Vitriol.* well cover'd with *Syr. Sambuc.*

The purple Spots diſappear'd, after he had taken two Drachms of the *Extract* (which nevertheleſs was continued on thro' the whole Diſeaſe). The Puſtules came on very favourably, and the Diſtemper afterwards, in its ſeveral Stages, more reſembled the diſtinct Small-Pox than the confluent: Yet I never ſaw one where the Puſtules were more numerous, and more truly confluent, not on the Face only, but the Arms and the whole Body: For, in many Parts, the Skin was raiſed for a conſiderable Extent by the included *Pus*, as if it had been done by a bliſtering Plaiſter; particularly the Arms from the Elbows to the Wriſts were intirely raw; the Skin, upon letting out the Matter, coming off like a Glove in one intire Piece.

About the ſame time I was call'd to a young Man of 21 Years of Age in the Pariſh of *Elmley-Lovat* in this County, who, during the Eruption of the Small-Pox (which appeared on the ſecond Day of his Illneſs), had a violent Hæmorrhage from the Noſe, Purples very numerous, and exceſſive Pain in

the

the Loins. He went well through the Diſeaſe by the ſame Method.——Neither of theſe two had any condary Fever.

In the Caſes above-recited I truſted to the *Bark* alone; excepting only that I acidulated the Liquors with the vitriolic Acid; but lately I have joined Alum with it, and I think to Advantage: For, as the chief Efficacy of the *Cort. Peruv.* ſeem'd to me to ariſe from its ſtyptic and antiſeptic Qualities, whereby it not only ſtrengthens the Solids, but gives a Firmneſs alſo to the Blood and Juices, and thereby prevents their degenerating into a putrid *Sanies*, I thought it might not unuſefully be joined with Alum, a Salt of ſingular Virtue in the ſame Intentions, and which is well known to aſſiſt the *Bark* in other Caſes, where it is order'd as a Styptic.

Mr. *Higgins*, of *Tedney* in *Herefordſhire*, a young Man of about 24 Years of Age, after very violent Exerciſe in hot Weather, was ſeized with the previous Symptoms of the Small-Pox. The Pain in his Back particularly was very violent, and his Anxiety intolerable. On the third Day of his Illneſs, when I firſt ſaw him, his Skin was cover'd with purple Spots, many of them as large as a ſilver Peny; and his Pulſe was ſmall, and very quick. Notwithſtanding this, as he was a ſanguine young Man, the Weather hot, and the Sickneſs came after violent Exerciſe, I ventur'd to order a ſmall Quantity of Blood to be taken from his Arm, which I found very tender and florid; and at the ſame time I directed for him as follows:

℞ Extr. Cort. Peruv. ℥ſs. Alum. crud. ℈ii. Aq. Cinnam. ten. ℥ vii. Syr. Cydon. ℥ i. M. cap. coch. ii. larga alternis horis.

He had emptied this Bottle the next Day at Noon; when I viſited him again, and found him much better; his Pulſe being full and regular, and the Anxiety and Pain in his Back nearly gone.

The Small-Pox now began to ſhew itſelf, and the Purples declined apace. The Urine which he had made the Day before they had imprudently thrown away before I came; but they informed me that it was extremely red, as if mix'd with Blood; ſomewhat reſembling, as they ſaid, the Water in which freſh Meat had been waſhed. He had had ſeveral Stools; ſo that, fearing leſt that Diſcharge might be too violent, in the next Preſcription I omitted the Alum, and ſubſtituted the *Terra Japonica* in its ſtead. This Mixture he continued through the whole Courſe of the Diſeaſe, and uſed no other Medicine; excepting only that I found it neceſſary to give him a quieting Draught in the Evenings, containing a little *Syr. e Mecon.* a few Grains of Alum, and a Scruple of the *Extr. Cort. Peruv.*

All the bad Symptoms ſoon diſappear'd intirely: He had no ſecond Fever, but got thro' the Diſeaſe very eaſily.

Mr. *Bradford*, a Farmer at *Claines*, near this Town, about 21 Years of Age, was ſeized after the ſame manner, and got very well thro' the Diſeaſe, tho' it was confluent, by the ſame Method. I did not ſee him till the ſixth Day of his Illneſs, and then

then his Juices were in ſo bad a State, that, beſides the *Petechiæ*, which were very numerous, a Place on his Shin-Bone, about the Size of a Crown-Piece, which had been bruiſed a Day or two before his firſt Seizure, was now gangren'd; and the Edges look'd of a very deep Red, as if the Corruption was ſpreading further; his Throat likewiſe was ſo ſore that he could ſcarce ſwallow at all.

I order'd him the ſame Mixture as above, *viz.* *Extr. Cort. Peruv.* ℥ſſ. *Alum. crud.* ℈ii. in about half a Pint of Liquid.

He took of this two large Spoonfuls every other Hour. It was not without great Difficulty that he got down the firſt two or three Doſes; but he had Reſolution, and perſiſted; and before he had finiſhed the Bottle his Throat grew better, and he ſwallowed tolerably well. The next Day the *Petechiæ* began to fade away, and ſoon after diſappear'd intirely; the Gangrene alſo ſtopt, and in a few Days ſeparated. He continued this Medicine thro' the whole Diſeaſe, and needed no other.

Soon after Mr. *Bradford's* Recovery, his Siſter (about 19 Years of Age) who liv'd with him, was ſeized with the confluent Small-Pox, attended with innumerable *Petechiæ* and purple Spots, together with a prodigious Hæmorrhage *ab Utero*, violent Pain in the Loins, a great Looſeneſs, and Dejection of Spirits. She took the *Bark* with Alum in the ſame Method, through the whole Courſe of the Diſeaſe, and recover'd.

The *Diarrhœa* here appear'd to me to proceed from the putrid State of her Juices; and therefore, notwithſtanding

notwithſtanding that, I continued the Uſe of the Mixture, and this with good Succeſs; for by it all the bad Symptoms were gradually mitigated, and, in a few Days, intirely carried off.

But one of the moſt remarkable Inſtances that has has come to my Knowledge, both of the Efficacy of the *Bark* in this terrible Diſtemper, and the whole Courſe of the Diſeaſe is the following.

A Servant Maid, belonging to one Mr. *Buttor*, of this Town, was ſeized with the previous Symptoms of the Small-Pox; but as the Pain in the Loins was exceſſive, the Diſeaſe was unhappily miſtaken for a Fit of the Gravel; and accordingly ſhe had been blooded plentifully, and taken Opiates and ſaponaceous Medicines. It was the ſecond Day of the Eruption when I was firſt call'd to her. The Puſtules were then extremely numerous, ſmall, and in Appearance like a violent Itch: Her whole Body, Arms, and Legs, were cover'd with broad Spots of a deep purple Colour, many of them as large as a Sixpenny-Piece; her Eyes were red, and full of Tears, and her Countenance expreſs'd a great Anxiety (a Symptom not eaſy to be deſcribed, but a terrible Prognoſtic in all acute Diſeaſes); her Throat was ſo ſore, that ſhe ſwallowed with the utmoſt Difficulty; by Fits ſhe was delirious, and vaſtly reſtleſs: She had a violent Diarrhœa, a prodigious uterine Hæmorrhage; and a very quick ſmall Pulſe; in ſhort, ſhe ſeem'd juſt on the Brink of the Grave.

I gave her the *Bark* with Alum, in as large a Quantity as ſhe could get down. In about 12 Hours her Throat grew better, and ſhe ſwallowed

tolerably well; after which ſhe took half an Ounce of the Extract of the *Bark*, and two Scruples of Alum in 24 Hours. This Medicine I continued, without Variation, for three or four Days, when the Alum made her ſo ſick, that I was obliged to leſſen its Quantity, continuing ſtill to give her the Extract as before.

Within this time the Purples had all diſappear'd; the Hæmorrhage was ſtopped, and the Looſeneſs conſiderably abated; the Puſtules came on well, and ſhe ſpit plentifully.

The Diſeaſe continued to proceed very well, till the ſixteenth Day after the Eruption; but then, growing tir'd of her Medicine, ſhe perſuaded her Nurſe to omit it; which ſhe did for about 24 Hours. During this time ſhe took but a very ſmall Quantity of Liquids; for, as ſhe ſeem'd to lie eaſy, and in a kind of dozing Sumber, her Nurſe thought ſhe ought not to diſturb her. It was prodigious to obſerve the Alteration occaſion'd by this Omiſſion and Neglect: Her Pulſe grew quick and weak, and the Humours acquired the higheſt Degree of Putrefaction; ſo that ſhe died, perfectly gangrenous, on the 20th Day.

Theſe are a few Inſtances, out of many others which I could produce, of the ſurpriſing Effects of this Antidote. Indeed I have given it to very many Perſons in the firſt Stages of the Diſtemper, where the *Petechiæ* have appear'd before, or as ſoon as the Puſtules of the Small-Pox; and to others in the Time of Maturation, where the Matter has been crude and watry; and, I can ſafely ſay, almoſt always with

with Succeſs. I now uſually continue it thro' the whole Courſe of the Diſeaſe, till, the Scabbing being perfected, I find it Time to cleanſe the firſt Paſſages; and ſometimes I order it in the Intervals betwixt Purging for ſome time longer, where I find the Solids weak, or the Humours thin and acrimonious.

When I am called to a Perſon, and, from the Appearance of *Petechiæ*, purple Spots, Hæmorrhages, miliary Eruptions, or the like, find that the Texture of the Blood is broke, or in Danger, I immediately order the *Bark*. Nor does the Quickneſs of the Pulſe deter me from its Uſe; on the contrary, I think it abſolutely neceſſary to give it, where the Pulſe is quick, if at the ſame time it be weak. Becauſe, for very obvious Reaſons, I judge that, in this Caſe, the Solids are weak, and the Fluids diſpoſed to a putrid Acrimony.

From the Caſes here recited, as well as from many others which I have met with, I think nothing more effectually or ſpeedily cures a ſore Throat in the Small-Pox than the *Bark*; nor did I ever find it at all check the Spitting in thoſe Sorts of Small-Pox where that Evacuation is neceſſary.

If, in the firſt Stages of the Diſeaſe, the *Bark* ſeems to run off by Stool, ſo far from being prejudicial, I have commonly found it of Service. For, as † *Hoffman* well obſerves, that a natural Looſeneſs often

† Alvi fluxus licet copioſus non adeo pertimeſcendus eſt ** ſc. ſicuti in febribus malignis petechialibus, alvi profluvium morbi facit ſolutionem; ſic

often carries off the *Petechiæ*, so that produced by the *Cortex* herein imitates the kindly Efforts of Nature; in that respect assisting the Physician in his principal Office, which is to be *Naturæ Minister*.

In most of those, to whom I have given the *Bark*, I have found the Maturation of the Pustules forwarded by it, and the Disease shortened in its Duration; an Article surely of no mean Consideration. I commonly use the Extract (made by boiling down a Decoction of the *Bark*, without the Addition of any alcaline Salt) preferably to the gross Substance; as being, I think, of equal Efficacy, and less apt to load the Patient's Stomach. In Children and delicate Persons, who are apt to nauseate this Remedy, I have with Success given it mix'd up with thin Chocolate; which, if sufficiently sweetened, disguises it better than any thing I know of.

What the *Bark* might do, as preventive, or preparative for this Disease I know not; but in the latter Intention I should think it would prove of Use.

I suppose I need not take notice, that when I recommend the *Bark*, I would not be thought to decry or discountenance the Methods or Medicines usually order'd in this Distemper; on the contrary, I think, in so terrible a Situation, nothing ought to be

sic etiam in mali genii variolis idem evenire experientia docet. *Hoffm. de feb. variolos. Thes. patholog.* § 12.

Alvi profluvium non nocet, quando petechiæ pariter ob valde irregularem anni constitutionem complicantur, ut potius remedio sit, et malignos humores optime expurget. *Id. ibid. Cautel.* § 16.

be omitted which can any way alleviate or aſſiſt the Patient. Dr. *Monro* has already made proper Remarks on this Head, and to his Words I refer the Reader.

I would not obtrude the foregoing Paper on the World, as containing any new Diſcovery; but as I think the Efficacy of all Medicines is beſt to be known from repeated Trials, and communicated by accurate and authentic Hiſtories of their Succeſs, on theſe Accounts, I thought proper to offer theſe Obſervations to the Public, hoping that they may at leaſt ſerve as a Commentary to the Papers already publiſhed on the Virtues of this juſtly celebrated Drug.

Worceſter, March 1(
1746-7.

T. Wal

V. *Postscript of a Letter from the Rev. Dr.* Doddridge *at* Northampton, *to Mr.* Henry Baker *F. R. S. of one, who had no Ear to Music naturally, singing several Tunes when in a* Delirium.

Read Nov. 12. 1747.

I HARDLY know whether it be worth while to mention a little Event that happened in our Neighbourhood some time ago, which yet appear'd to me something singular in its kind.

A Clergyman's Lady, whose Husband is of some Eminence in the learned World, in a Frenzy after a Lying-in, which was quickly removed, found, during the Time of it, such an Alteration in the State and Tone of her Nerves, that, whereas she never had before or since any Ear for Music, nor any Voice, she was then capable of singing, to the Admiration of all about her, several fine Tunes, which her Sister had learned in her Presence some time before; but of which she had not then seemed to take any particular Notice.

Northampton, Nov. 3. 1747.

VI. *A Catalogue of the* Fifty Plants *from* Chelſea-Garden *preſented to the* Royal Society *by the Company of* Apothecaries *for the Year* 1745. *purſuant to the Direction of Sir* Hans Sloane *Bart. Med. Reg. et Soc. Reg.* nuper *Præſ. by* Joſeph Miller *Apothecary. Hort.* Chelſ. *Præf. et Prælect. Bot.*

Read Nov. 12. 1747.

1151. ACorus verus, ſive Calamus aromaticus. *Off.* et *C. B.*

1152. Aconitum Lycoctonum cœruleum calcari oblongo. *J. B.*

1153. Adonis aliàs *Eranthemum.* ibid.

1154. Agrimonia *Orientalis*, ſpica brevi craſſa *&c.* *Tourn.*

1155. Amaranthus *Siculus* ſpicatus. *Boccone.*

1156. Ariſtolochia, Clematitis recta. Off. et *C. B.*

1157. Aſter *Atticus* cœruleus vulgaris. ibid.

1158. Balſamina fœmina. *C. B.*

1158. Bellis radice repente, latioribus ſerratis foliis. *Moriſon.*

1160. Bidens foliis non diſſectis. *Tourn.*

1161. Carthamus. *Off.* flore croceo. ibid.

1162. Catanance flore luteo latifolia. ibid.

1163. Caryophyllus *Sinenſis* flore vario. *Rand. Hort. Chelſ.*

1164. Chamæceraſus *Alpina* fructu gemino rubro. *C. B.*

1166. Chryſanthemum *Bermudenſe*, Leucoii folio craſſo. *Pluk.*

1166. Cir-

1166. Cirſium *Anglicum*. *Ger.*
1167. Ciſtus fœmina Salviæ folio. *C. B.*
1168. Ciſtus mas *Luſitanicus* folio ampliſſimo incano. *Tourn.*
1169. Ciſtus Ladanifera *Hiſpanica* Salicis folio. *C. B.*
1170. Cotyledon *Africana* fruteſcens, flore coccineo umbellato. *Comelin.*
1171. Cytiſus *Alpinus* flore luteo racemoſo pendulo.
1172. Emerus, *Tourn.* Colutea ſcorpioides. *Park.*
1173. Ephedra major maritima. *Tourn.*
1174. Elichryſum ſpicatum. ibid.
1175. Fritillaria præcox purpurea variegata. *C. B.*
1176. Fritillaria alba præcox. ibid.
1177. Galega vulgaris floribus cœruleis. ibid.
1178. Gentiana. *Offic.* major lutea. *C. B.*
1179. Glycyrrhiza ſylveſtris flore luteo pallido. ibid.
1180. Helleborus niger, folio Ranunculi flore globoſo. *Tourn.*
1181. Jaſminum ſive *Sumbach* Arabum. Alpini. *J. B.*
1182. Lamium rubrum minus foliis profunde inciſis. *Raii Syn.*
1183. Lepidium latifolium. *Off.* et *C. B.*
1184. Lotus rubra ſiliqua anguloſa. *C. B.*
1185. Lyſimachia lutea major quæ *Dioſcoridis*, Off. et *C. B.*
1186. Medica orbiculata. *J. B.*
1187. Melilotus Offic. et *C. B.*
1188. Mirabilis *Peruana* flore variegato. *Park. Parad.*
1189. Oenanthe Staphilini folio aliquatenus accedens. *J. B.*

1190.

1190. Oenanthe Apii folio. *C. B.*
1198, Papaver hortensis semine nigro sylvest. *Dioscorid.* ibid.
1892. Rawolfia tetraphylla latifolia. *Plumier.*
1193. Reseda calcitrapæ folio. *Morison.*
1194. Rosa sylvestris pomifera nostras. *Raii Syn.*
1195. Salvia major; an Sphacelus *Dioscorid?* *C. B.*
1196. Salvia minor aurita et non aurita. ibid.
1197. Scabiosa arborea Cretica. *Ponæ.*
1198. Statice foliis angustioribus flore rubro. *Tourn:*
1199. Stœchas purpurea. Off. et *C. B.*
1200. Thlaspi amarum arvense umbellatum. *J. B.*

VIII. *A Continuation of an Account of an Essay towards a* Natural History *of* Carolina *and the* Bahama Islands; *by* Mark Catesby *F. R. S. with some Extracts out of the tenth Set, by* Cromwell Mortimer *Secr. R. S.*

Read Nov. 19. 1747.

THE Abstract of the preceding Set, which I laid before this *Society* in the Year 1738. is printed in the *Philosophical Transactions* N°. 449. This tenth Set begins with Plate 100, of the second Volume. In this Part of the Work, the Author, besides Plants, has given us several Insects, particularly some remarkable Butterflies. He begins this Set with the *Mahogony-Tree*, whose Wood is of late Years become so well known here in *England*, for all sorts of Joyner's Work,

 fur-

ſurpaſſing the red Cedar in Beauty, without having the diſagreeable Scent of that Wood.

81. *Arbor foliis pinnatis, nullo impari alam claudente, nervo ad latus unum excurrente, fructu anguloſo magno, ſemine alato inſtar Pinus.* The Mahogony-Tree.

Theſe Trees grow to a great Height, and are uſually four Foot Diameter; the Seed-veſſels are of a curious Form, conſiſting of a large Cone ſplitting into five Parts, and diſcloſing its winged Seeds, diſpoſed in the regular manner of thoſe of an *Apocynum:* And at the *Bahama* Iſlands, and other Countries where it grows naturally, it is in no leſs Eſteem for Ship-Building, having Properties for that Uſe excelling Oak, and all other Wood; *viz.* Durableneſs, reſiſting Gun-ſhots, and burying the Shot without ſplintering. No one would imagine that Trees of this Magnitude ſhould grow on ſolid Rocks, and that thoſe Rocks ſhould afford ſufficient Nutriment to raiſe and increaſe the Trunks of them to the Thickneſs of four Feet or more in Diameter; but ſo it is; and the Manner of their Riſe and Progreſs the Author hath obſerved as follows: The Seeds being winged are diſperſed on the Surface of the Ground, where ſome falling into the Chinks of the Rocks, ſtrike Root; then creep out on the Surface of it, and ſeek another Chink, into which they creep and ſwell to ſuch a Size and Strength, that at length the Rock ſplits, and is forced to admit of the Root's deeper Penetration; and with this little Nutriment the Tree increaſes to a ſtupendious Size in a few

it being a quick Grower.

Viſcum

Viscum foliis longioribus, baccis rubris.

This red-berried *Misleto* grows on the Bark of the *Mahogony* and some other Trees, as our *Misleto* does on Apple-Trees.

82. *Bignonia Americana, capreolis donata; siliqua breviore. Tournefort. Inst.* This elegant Plant endures our Climate.

[In the following PLATES the Author has interspersed several remarkable Butterflies, and other Insects.]

83. *Frutex Virginianus trifolius, Ulmi samaris, Banisteri.* Pluk. Alma. 159. This agrees with our Climate.

Papilio caudatus maximus, Carolinianus, umbris striisque nigris. Pet. Muf. N°. 505.

84. *Philadelphus flore albo majore inodoro.* This agrees with the Climate of *England.*

Smilax non spinosa baccis rubris.

Phalæna plumata caudata, Caroliniana virescens oculata. Pet. Muf. *p.* 69. N°. 733.

85. *Anona fructu lutescente, levi, scrotum Arietis referente.*

There are many Species of this *Genus* growing between the Tropics; but this only is to be found on the Northern Continent of *America.*

86. *Anona maxima, foliis oblongis angustis; fructu maximo luteo conoide; cortice glabro in areolas angulares distincto.*

Phalæna magna, ex rufo et albo varia, Americana.

87. *Anona foliis Laurinis, in summitate incisis; fructu compresso scabro fusco, in medio acumine longo.* The *Sappadillo* Tree.

Convolvulus foliis variis interioribus trifariam, diviſis, ſuperioribus ſagittatis; floribus ex rubro purpureis.

88. *Viſcum radice bulboſa; floris labello carneo, ceteris petalis ſordidè luteis.*

This bulboſe-rooted Plant grows only to the Trunks, and on the Limbs of Trees. Its Fibres inſinuating into the Crevices of the Bark where they take ſuch firm Rooting that great Strength is required to tear them from the Trees. They grow in the *Bahama* Iſlands.

Viſcum Cariophylloïdes, Lilii albi foliis; floris labello brevi purpureo, cæteris petalis ex luteo vireſcentibus.

Theſe Plants, after the manner of the precedent, grow upon Trees on bulboſe Roots, in the *Bahama* Iſlands.

Papillio rufa marginibus nigris punctis albis notatis.

89. *Viſcum Cariophylloïdes anguſtifolium; floribus longis tubuloſis cæruleis, ex ſpicis ſquamoſis cæruleis erumpentibus.*

This Plant, tho' bulboſe-rooted, grows to the Limbs and Branches of Trees. The Leaves are concave; the whole Plant reſembling ſomewhat the *Ananas*. What recommends this uſeful and very ſingular Plant is, that its hollow Leaves, lapping over one another, are ſo cloſely placed, that one Plant will contain two Quarts of clear Water. In many Countries between the Tropics, that are deſtitute of Water, having neither Springs nor Rivers, theſe Plants abound, and are of great Benefit in relieving

the thirsty Traveller, as (says our Author) I have often experienced in Draughts of this refreshing Water; which, tho' receiving the Heat of the Sun's perpendicular Rays, was always as cool as from a Spring. These Plants are common on many of the *Bahama* Islands, and usually grow on large Trees; particularly *Mahogony*, *Sappadillo*, *Mançanilla*, &c. which are sometimes so cover'd with them, that they seem to be the Leaves and Blossoms of the Trees on which they grow, and make a very elegant Appearance.

Locusta Caroliniana, elytris fuscis; alis interioribus nigris ad extremitates luteis.

90. *Ketmia, amplissimo Tiliæ folio subtus argenteo, flore magno luteo.* The *Maho* Tree. Of the inner Bark of this Tree the *Musketo Indians* make their Lines both for Fishing and Striking, it being very tough and durable: It is also of great Service to the *American* Privateers, who make their Cordage and Rigging of it. (See more in Sir *Hans Sloane*'s *Hist. Jam.* Vol. I. p. 215.)

Phalæna fusca, alis superioribus Lunulis nigris notatis, inferioribus lunatis et oculatis iridibus sulphureis.

91. *Caryophyllus spurius inodorus, folio subrotundo scabro, flore racemoso hexapetaloide coccineo speciosissimo.* Hist. Jam. *Vol.* II. *p.* 20. *T.* 164.

Convolvulus minor pentaphyllos, flore purpureo minore.

Phalæna ingens Caroliniana oculata è luteo fusca, lineis dilute purpureis insignita. The *Great Moth.*

92. *Plumeria flore roseo odoratissimo. Tourn. Inst.* This is a most elegant Plant of the *Nerium* Kind,

Kind, in great Esteem in Gardens for its Smell and Beauty.

92. *Plumeria flore niveo, foliis brevioribus obtusis.* Plum. Cat.

Granadilla, foliis Sarsaparillæ trinerviis; flore purpureo: fructu Olivæformi cæruleo. The *Purple Passion-flower.*

94. *Cerasus latiore folio; fructu racemoso purpureo majore.* The *Pidgeon-Plum.* The Fruit is ripe in *December*, is pleasant-tasted, and is the Food of Pidgeons, and many wild Animals.

Eruca maxima cornuta. Hist. Jam. *Vol.* II. *p.* 220. The great horned *Catterpillar.*

95. *Mancanilla Pyri facie.* Plumier. Plant. Americ. *Juglandi affinis arbor Julifera,* &c. Hist. Jam. *Vol.* II. *p.* 3. The *Manchaneel* Tree. This Wood is much esteem'd for Tables, Cabinets, and other curious Works in Joinery; but the virulent and dangerous Properties of the Sap cause a general Fear, or at least Caution, in felling them. This, says the Author, I was not sufficiently satisfy'd of, till assisting in the cutting down a Tree of this kind on *Andros* Island, I paid for my Incredulity: Some of the milky poisonous Juice spirting in my Eyes, I was two Days totally deprived of Sight, and my Eyes and Face much swell'd, and felt a violent pricking Pain the first twenty-four Hours; which from that Time abated gradually with the Swelling, and went off without any Application, or Remedy, none in that uninhabited Island being to be had. It is no Wonder that the Sap of this Tree should be so virulent, when Rain or Dew falling from its Leaves on the naked Flesh causes Blisters on the

Skin,

Skin, and even the *Effluvia* of it are ſo noxious as to affect the Senſes of thoſe which ſtand any time under its Shade. Other malignant Effects are commonly attributed to it, but I think with little Probability. One Charge of its pernicious Quality is, that Animals, which feed on the Fruit, are ſo infected by it, that Death is often the Fate of thoſe that feed on ſuch Animals. This is refuted in the Inſtance of *Guana's*, *p*. 64. *Vol.* II. The Report alſo of the *Baracauda's*, and other Fiſh, receiving their poiſonous Quality by feeding on *Mançaneel* Apples, is likewiſe erroneous.

Viſcum foliis latioribus, baccis purpureis pediculis inſidentibus.

Papilio medius Gadetanus, ex nigro et ſulphureo varius, maculis coccineis notatus.

96. *Prunus maritima racemoſa, folio rotundo glabro; fructu minore purpureo.* Hiſt. Jam. *Vol.* II. *p*. 129. The *Mangrove Grape* Tree. This is a very ſpecious Tree, producing ample ſtiff Leaves, on both Sides of which the *Spaniards* uſed to write with a Bodkin, when they were in want of Pen, Ink, and Paper. It produces a purple-colour'd pleaſant Fruit reſembling a Plum, the Stone of which is very aſtringent, and is uſed in Fluxes, with great Succeſs. The Wood of this Tree makes a ſtrong Fire, therefore uſed by the Privateers of *America* to harden the Steels of their Guns when faulty.

Phalæna Caroliniana minor, fulva; maculis nigris alba linea, pulchre aſperſis. Pet. Gaz. Nat. *Tab.* III. *Fig.* 2.

97. *Acacia foliis amplioribus; ſiliquis cincinnatis.* Plum. Cat.

Papilio

Papilio diurna prima, omnium maxima. Mouffet *p.* 98. Raii Hist. Insect. *p.* 111. Mamankanois in M S. Dni. Gualteri Raleigh penes D. Hans Sloane.

98. *Chamædaphna foliis Tini, floribus bullatis umbellatis.*

As all Plants have their peculiar Beauties, it is difficult to assign to any one an Elegance excelling all others; yet, considering the curious Structure of the Flower, and beautiful Appearance of this whole Plant, I know of no Shrub (says our Author) that has a better Claim to it; but the noxious Qualities of it lessen that Esteem which its Beauty claims: For, tho' the Deer feed on its green Leaves with Impunity, yet when Cattle and Sheep, by severe Winters deprived of better Food, feed on the Leaves of these Plants, a great Number of them die annually on the Continent of *America.* —— After several unsuccessful Attempts (says the Author) to propagate it from Seeds, I procured Plants of it at several times from *America*, but with little better Success; for they gradually diminished, and produced no Blossoms; till his curious Friend Mr. *Collinson*, excited by a View of its dried Specimens, and Description of it, procured some Plants of it from *Pensilvania*; which Climate being nearer to that of *England* than that from whence mine came, some Blossoms were produced in *July* 1740, and in 1741, in my Garden at *Fulham.*

99. *Cenchramidea arbor saxis adnascens, obrotundo, pingui folio; fructu pomiformi, in plurimas capsulas, granula ficulnea stilo columnari octogono [illegible] adhærentia continentes, diviso; Balsamum fundens.* Plum. Almag. The *Balsam* Tree. This Plant

Plant in *June* produces ample fair Flowers, composed of six white Petals stained with purple, surrounding the Rudiment of the Fruit, which is almost spherical, and increases to the Size of a midling Apple. From the Stalk to the Crown of the Fruit run eight Lines, like the Meridians on a Globe, from Pole to Pole. When the Fruit becomes ripe it opens at these Lines, and divides into eight Parts, disclosing its mucilaginous scarlet Seeds, which are contained in the hollow Furrows of an octagonal Core. The whole Plant is exceeding beautiful, and particularly the Structure of the Fruit in all its Parts is a most excellent Piece of natural Mechanism.

These Trees grow on Rocks, and frequently on the Limbs and Trunks of Trees, occasioned by Birds scattering or voiding the Seeds; which being glutinous like those of Missleto, take Root and grow; but not finding sufficient Nourishment to increase in Growth, the Roots spread on the Bark or Superficies of the Tree, till they find a decay'd Hole, or other Lodgment wherein is some Portion of Soil, into which they enter, and become a Tree: But the Fertility of this second Plantation being exhausted, one or more of the Roots are discharged out of the Hole, and fall directly to the Ground, tho' at forty Feet Distance, Here again they take Root, and become a much larger Tree than before. The Resin of this Tree is used for the Cure of Sores in Horses, and also instead of Tallow, for Boats and other Vessels. They grow on the *Bahama* Islands, and on many other of the hot Parts of *America*.

100. *Frutex ſpinoſus buxi foliis, plurimis ſimul naſcentibus; flore tetrapetaloide, pendulo, ſordide flavo, tubo longiſſimo; fructu ovali croceo, ſemina parva continente.*

The Leaves of this little Tree were like thoſe of Box; the Flowers were tubulous, of a yellow Colour about ſix Inches long, hanging pendulous: They were monopetalous, being very ſmall at the *Calix*, and wide at the Mouth, in Form of a *Roman* Trumpet, except that their Verge was divided into four deep Segments, reflected back.—Shewing a Specimen of this Plant to Conſul *Sherard*, who was ſo juſtly celebrated for his Knowledge in Plants, he expreſſed his Admiration, and declared, that had he not ſeen the Thing itſelf, he could not have believed there had been ſuch a ſingular Plant in Nature.

[Dr. *Gronovius*, a very learned Gentleman at *Leyden*, and curious in botanical Studies, has paid our Author the Compliment of giving this Plant the Name of *Catesbæa*. *]

Papilio caudatus Carolinianus; fuſcus, ſtriis palleſcentibus; linea et maculis ſanguineis ſubtus ornatus. Pet. Muſ. *p.* 50. *N.* 508.

* As *Langrenus* and *Ricciolì* diſtinguiſhed the Regions in the Moon by the Names of Men famous in Philoſophy and Mathematicks, ſo have the Botaniſts paid their Compliments to Men eminent for their Knowlege of Plants, or for being Encouragers of Botany, by giving their Names to new-diſcover'd Plants.

VIII.

VIII. *An Account of the Death of the Reverend Dr.* Greene, *late Rector of* St. George *the* Martyr *in* Queen's Square London, *and one of the* Prebendaries *of* Worcester, *where he died of an Hurt received, as he was riding out in the Neighbourhood of that City, contain'd in a Letter from* Tho. Cameron *M.D. to the Reverend* Charles Lyttelton *LL.D. and F.R.S.*

SIR,

Read Dec. 10. 1746-7.

ON *Tuesday* the 20th of *October*, about Noon, Dr. *Greene*'s Horse, strong, nimble, and vitious, started under him, at the waving of a Plowman's Whip, and with a quick and violent Jerk, turned quite short, first to the left, and then instantly, and with the same Impetuosity, to the right. After galloping a few Paces, the Doctor fell gently off into a Hedge, without receiving any Hurt from the Fall. A Chariot was borrowed, into which he was lifted and brought home; for after this he could never stand.

At four that Evening I first saw him, just after he had been blooded. He was very faint, cold all over, and his Pulse scarce perceptible, tho' naturally very strong; the *Scrotum* so much swell'd, that the *Penis* was quite absorbed and lost in it, and its Colour a very deep Red. I ordered him a Glass of Wine with a Bit of Bread, for he had eat nothing

all that Day. This revived him, and raised his Pulse a little.

He then told me, in Answer to the Questions I put, that the Testicles were not hurt; that the twisting of the Horse gave him at that Instant the intolerable Sense of being split asunder. I answer'd, that a violent and sudden Stroke, from the Pummel of the Saddle, upon the *Os Pubis*, might probably give him that Sensation: He replied, that it did not feel like a Stroke, and still persisted in his first Expression of being split asunder.

A warm Fomentation was order'd to be constantly applied, and an Ounce and half of *Glauber's* Salt, quicken'd with two Grains of Emetic Tartar, to be given in a Quart of Gruel.

At Eleven that Night an emollient Glyster was given, the Salts having as yet done nothing. Before Morning he had six large loose Stools; but it gave him exquisite Pain to be lifted upon the Bed-pan.

Next Morning, *Wednesday*, the Swelling was increased, and the Colour deeper. I prescribed an Electuary of Bark and Salt of Amber, to prevent, if possible, the approaching Mortification.

The stale Beer Poultice was applied; and that Evening, a Fever coming on, ten Ounces of Blood were taken from his Arm.

Next Morning, *Thursday*, the Salts were repeated without the Emetic Tartar, and he had four Stools.

All this while he had made no Water, except about a Spoonful just after he was put into the Chariot. The lower Part of the Belly, where a distended Bladder would certainly point, was not swell'd, tho' the Parts upon the *Os Pubis* were very much so. The *Scrotum* increased in Bulk and bad Colour every Hour; and the Inside of the right

Thigh

Thigh grew very tumid, with great Pain, and a very perceptible Fluctuation in it.

These Observations convinced me that the Urine had found a Way into the Parts last-mention'd; tho' indeed I could not account for it, but by supposing that the *Urethra* had been bruised, even to Laceration, between the Pummel of the Saddle and the *Os Pubis*,

The Surgeon, Mr. *Russel*, soon came into my Opinion about the Urine, but imagined the Bladder must be burst. This I could not comprehend, nor could he explain; for the Bladder lies out of the Reach of all external Injury from the Causes hitherto assigned in this Case. We agreed however about Three in the Afternoon, *Thursday*, to make a Puncture into the *Scrotum*; from whence Urine, manifest to the Smell, issued pretty freely all Night.

Next Morning, *Friday*, a larger Opening was made in the right Thigh with the same Effect. The Parts subsided considerably; but the Pulse rising, *Decoctum nitrosum* was given with the Bark.

This Evening the Hiccup came on, and the *Scrotum* looked livid.

Next Day, *Saturday*, the common Emulsion, with a little Nitre, and the Extract of Bark with Musk were order'd. But the Hiccup increased, watry Blisters appear'd on the *Scrotum*, the Voice faulter'd, the Head failed, and the Pulse sunk. He grew worse and worse, till he quietly expired on *Sunday* Morning at Eleven o' Clock.

Upon Dissection, we found the *Scrotum* and *Corpora cavernosa Penis* mortified; the *Ossa Pubis* wrenched asunder to the Distance of four Inches, and

and a Rent in the Bladder, half an Inch in Length, a little above the Neck, and exactly in the middle where the *Offa Pubis* join. This was a very aftonifhing Sight, and gave me quite a new Idea of the Cafe; which if any Phyfician could have difcover'd without Infpection, I fhall readily allow him more Penetration than I pretend to. We may now however, reafon about it, with a little more Certainty than before; and it feems to me, that the Body of the Horfe in twifting, acted with the Power of a Lever, to which the Suddennefs of the Jerk, added in fome meafure the Force of Percuffion. But all this leaves us ftill in Wonder at the Effect: For Dr. *Greene* was a very ftrong large-boned Man, fixty-four Years of Age, and the uniting Surface of the *Offa Pubis* was confiderably broader in him, than either the Surgeon, or I had ever feen in any Subject. I fear I have tired you; but am,

SIR,

Worcefter, Dec. 2. 1747.

Your moft obedient

humble Servant,

Thomas Cameron.

IX. *A Letter from the Reverend* Henry Miles *D. D. to Mr.* Henry Baker *F. R. S. concerning the Difference of the Degrees of* Cold *marked by a* Thermometer *kept within Doors, or without in the open Air.*

Dear Sir,

Read Dec. 10. 1747.

I SEND you herewith an Extract from my Register of the Weather, shewing the State of my Barometer and Thermometers, for some Days of last Week: in which you will observe a sudden Change of the Temperature of the Air, particularly on *Thursday* Morning the 3d Instant, and, by the same, you may see the little Use a Thermometer is of, when kept within-doors, to determine the State of the Air abroad, as to Heat or Cold.

I have two Thermometers filled with *Mercury*, and of the same Construction, made by the late Mr. *Sisson*, in the *Strand*. The one is placed without my Chamber-Window, in a North-east Situation, under Covert, contriv'd to admit a free Passage of the Air, but to keep off Sun and Rain; the other hangs within the Window, about three Feet from the former, where the Sun never falls on it: The Room is constantly occupied, as a Bed-Chamber, but has had no Fire in it this Season.

It appears by the adjoining Table, that on *Tuesday* the 1st instant, at 8 in the Morning, the Thermometer without stood at 17 Degrees above 0. or freezing Point; that within at 14. At 9 at Night, that without was at 0. and that within at 12 above

0.

o. So that in the Space of 13 Hours the former had fallen 17 Degrees, the latter but 2. For the other Particulars, relating to the Barometer, Wind, and Weather, I refer to the Table.

As the Barometer had been for a good while paſt ſubject to ſudden conſiderable Variations, I ſuſpected the ſevere Cold on *Wedneſday* Night and *Thurſday* Morning would not continue long: Accordingly, upon my obſerving the Thermometer without at 4 in the Morning, I found it at $\frac{0}{9}$ nine Degrees below the freezing Point, that within at $\frac{5}{0}$ five Degrees above freezing Point. But at 8 o' the Clock the ſame Morning, I found the Thermometer without at $\frac{3\frac{1}{2}}{0}$ three Degrees and a half above freezing, and that within at 4 Degrees above; ſo that in 4 Hours time, that without had riſen thirteen Degrees and a half, and that within had fallen 1 Degree. This naturally led me to examine what Signs there might be of a Thaw begun, but could find none, in the Snow (which was 5 Inches deep) or in the Poſt, on the Windows, but within an Hour it was viſible enough, and before 10 the Houſes dropt. I would obſerve to you, that the Wind at 8 in the Morning had varied very little, if any, from what it was the Night before, *viz.* from the Eaſt, but ſoon after it bore to South-Eaſt and South.

May not this ſudden Change of the Temper of the Air be attributed it to a ſubterranean Heat? And may not the ſhifting of the Wind be cauſed, in a great meaſure, by the ſame?

If you think theſe Obſervations may be acceptable to the Gentlemen of the *Royal Society*, who keep

a Regiſter of the Weather, and may ſerve to perſuade thoſe, who have not yet tried it, to hang their Thermometers abroad, you have Leave to communicate it from their and

Tooting, Dec. 8. 1747. *Your moſt obedient Servant*,

Henry Miles.

Days	Morning Barom.	Morning Ther.	Evening Barom.	Evening Ther.	December 1747.
1	28 6 3	$\frac{17}{.0}$ $\frac{14}{0}$	29 3 0	0 $\frac{12}{0}$	At 8 Morn. Wind high at S. W. much Rain preceding Night. Showery afterward in the Morning, and Wind exceeding high. Sleet at $1\frac{1}{2}$ *p. m.* calmer and clearer soon after, Wind N. W. and N. began to freeze in the Evening, clear at $9\frac{1}{2}$ *p. m.* when the Evening Account was set down.
2	29 5 5	$\frac{0}{5}$ $\frac{9}{0}$	29 4 0	$\frac{0}{2}$ $\frac{5}{0}$	At 8 Morn. cloudy thick Air, hard Frost; at 4 same Morn. very clear, and Glasses were at 29 5 0 and $\frac{0}{3}$ and $\frac{4}{0}$ Wind East, cloudy all Day, at $4\frac{1}{2}$ *p. m.* Snow fell, and was deep before 8. Evening Account taken at $9\frac{1}{2}$ *p. m.*
3	29 5 6	$\frac{3\frac{1}{2}}{0}$ $\frac{4}{0}$	29 4 9	$\frac{13\frac{1}{2}}{0}$ $\frac{9}{0}$	At 8 Morn. cloudy, Wind at near East; blows brisk at 4 the same Morn. Glasses were 29 6 1 $\frac{0}{9}$ and $\frac{5}{0}$ Rain before 11. Evening Account at $9\frac{1}{2}$ *p. m.*

Explication.

1 Day Barom. Morn. 28 Inches $\frac{6}{10}$ $\frac{3}{100}$.
Ditto. Therm. Morn. $\frac{17}{0}$ is 17 Degrees above freezing Point the upper Number is for the Therm. without Doors, the lower for that in my Room, and so for the rest.

λ.

X. *An Account of a* Child *being taken out of the* Abdomen, *after having lain there upwards of* 16 *Years, during which Time the Woman had* 4 *Children, all born alive; by* Starkey Myddleton *M. D.*

Gentlemen,

Read Dec. 17. 1747.

THE Records of your *Society* furniſh us with ſeveral Caſes of extra-uterine Conceptions; one of which I communicated to you *March* 28. 1745.* nevertheleſs I could not help flattering myſelf that this Caſe alſo might be worthy your Notice.

IN *April* 1731. Mrs. *Ball,* without *Biſhopſgate,* perceived (by the uſual Symptoms) that ſhe was pregnant; and in *October* following, being then in the ſixth Month of her Pregnancy, ſhe had a Child died in her Lap of Convulſions, the Surprize of which occaſion'd a great Fluttering within her, attended with a ſenſible Motion of the Child, which Motion continued, tho' gradually weaker and weaker, for about 6 or 7 Days, after which ſhe did not perceive it to move any more; but from this time ſhe had conſtant Pains attending her, which appear'd like Labour-Pains. Her Midwife, for ſeveral Days, expected a Miſcarriage; but finding herſelf diſappointed, advised her to apply to Dr. *Bamber,* whoſe known Abilities in the ſeveral Branches of Phyſick, joined to his great Experience and Judgment in

 Midwifry,

* See *Philoſ. Tranſ.* No. 475. p. 336.

Midwifry, made him unquestionably the most proper Person to be consulted, as the Case appear'd so very uncommon in its Circumstances, at the same time that his great Humanity always gave the most free Access to the Poor in their Distresses.

The Doctor (after a proper Examination) finding sufficient Indications of a dead Child, order'd her some forcing Medicines; upon taking which about three times she discharg'd something, which the Women suppos'd to be Part of an After-birth, accompanied with a small Quantity of Water: In consequence of this Discharge her Pains ceased, but without any visible Diminution of her Belly.

After some time she again apply'd herself to the Doctor, who thought it most adviseable to discontinue her Medicines, and leave the Affair intirely to Nature.

In this State she continued for about 20 Months *viz.* to *July* 1733. which was 2 Years and 3 Months from her first Reckoning, she then again apply'd herself to Dr. *Bamber*, acquainting him, that she was not yet deliver'd of the Child she so long since came to consult him about, and that her Pains were lately return'd, and daily increased without any Intermission, upon the Doctor's examining her, he thought it proper to send her home immediately, directing her to promote her Pains, by frequently sipping some warm Caudle, *&c.* by the Use of which her Pains became more regular, and the next Day the Doctor made her a Visit, and was informed she had discharg'd two Waters, but nothing more: He then carefully examined her again, and plainly felt a Child through the Integuments of the *Abdomen*, but could not give her any Assistance.

It was about this time that Dr. *Bamber* firſt acquainted me with the Caſe, and deſired that I would attend her as often as Occaſion might require, and that I would acquaint him, if any thing like Labour, or other remarkable Alteration ſhould offer. Accordingly I made her a Viſit, and after a proper Examination, was convinc'd of the Certainty of the Doctor's Aſſertion.

Her Pains now began to abate, and ſhe grew tolerably eaſy; but about the latter End of *January* 1733-4. ſhe conceived again with Child, and was deliver'd on the 28th of *October* following by Dr. *Bamber*, who ſent for me to attend him in her Labour: The Doctor ſoon deliver'd her of a fine Boy, and after having brought away the *Placenta*, he ſearch'd for the other Child, which he had before felt through the Integuments of the *Abdomen*, but found it was lodg'd in the Cavity of the *Abdomen*, and beyond the Reach of human Art to relieve her. This Fact every one then preſent was made ſenſible of.

October 22. 1735. I was ſent for to her in her Labour, but before my Arrival ſhe was deliver'd of a Boy; but I brought away the *Placenta*, which gave me an Opportunity of examining for the other Child, and fonnd it in the ſame Situation as formerly.

October 9. 1738. I was again ſent for to her, when in Labour, but ſhe was deliver'd of a Boy before I arrived. Upon examining the Womb, and the State of the *Abdomen*, the Child appear'd juſt as before, without any Alteration.

June 17, 1741. I was again ſent for in her Labour, but found her juſt deliver'd of a Girl; and

upon

upon examining the Parts, every thing appear'd as before.

October 14. 1747. being greatly emaciated by constant Pains, *&c.* she was admitted a Patient in *Guy*'s *Hospital*, where she died the 7th of *November* following, after having labour'd under the Distresses and Uneasiness of carrying a dead Child within her, in a manner loose, in the Cavity of the *Abdomen* upwards of 16 Years.

The Day after her Death I open'd her, in the Presence of Dr. *Nesbit*, Dr. *Nichols*, and Dr. *Lawrence*, when the *Uterus*, and the several other Contents of the *Abdomen* appear'd (nearly) in their natural State, but on the right Side within the *Os Ilium* a Child presented itself, which was attach'd to the *Ilium* and neighbouring Membranes by a Portion of the *Peritonæum*, in which the *Fimbria* and Part of the right *Fallopian* Tube seem'd to lose itself.

The Child seem'd no-ways putrid; but the Integuments were become so callous, and chang'd from their natural State, that the whole seem'd to resemble a cartilaginous Mass, without Form or Distinction: The Legs indeed were distinguishable, though they were much wasted and distorted.

Upon opening the callous Integuments of the Head and Face of the Child, the Bones appear'd perfectly form'd, with a few Spots of tophous Concretions on them.

This Account may serve to convince those who are of Opinion that Boys are conceived on the right Side, and the Girls on the left; as this Woman

man had three Boys, and one Girl, after the *Fallopian* Tube on the right Side had lost its Action. I am,

Gentlemen,

With great Respect,

Decem. 17. 1747.

Your most obedient and

Most humble Servant,

Starkey Myddleton.

XI. *A Physiological Account of the Case of* Margaret Cutting, *who speaks distinctly, tho' she has lost the* Apex *and* Body *of her* Tongue: *Addressed to the* Royal Society, *by* James Parsons *M. D. F. R. S.*

Gentlemen,

Read Dec. 17. 1747.

AS several of the Members of this worthy *Society* were somewhat divided in their Opinions concerning what was reported of *Margaret Cutting*, when they were first informed of her by Mr. *Baker* *; it will be necessary (in order to render her Case the better understood) to lay before you the following short Particulars, which

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* *See these* Transf. N°. 464, Artic. II. *p.* 143, *et seq.*

which are the Refult of an Examination made a few Days fince by Dr. *Milward* and myfelf; and which, in general, differs not from the Opinion which that learned Gentleman and I mention'd to this *Society*, upon the Occafion, which the Science of Anatomy neceffarily fuggefted to us at that time. But *James Theobald* Efq; a worthy Member of the *Royal Society*, having encouraged her to come to *London*, and having brought her to this Meeting of the *Society*, has now given us all an Opportunity of coming at the Truth of her Cafe; wherefore I fhall now, Gentlemen, prefent you with, *firft*, an Account of her prefent Condition; and then, fome Confiderations on the natural State and Ufes of the Tongue; which will fhew you how far fhe makes the *Lips* and *Teeth* fupply the Want of *her* Tongue in fpeaking; and alfo be a Direction to every Gentleman prefent to judge of the Cafe before him.

Of her prefent Condition.

THE *Apex* and *Body* of the Tongue (being the only Parts that naturally fill the Cavity of the Mouth) are intirely wanting in this Woman, as clofely to the Region of the *Os Hyoides*, which is the Root of the Tongue, as can well be conceived; and which is now fituated too low in the Throat to be perceived, even when fhe opens her Mouth at the wideft.

But let any one lay the Tops of the Finger and Thumb to the Sides of her Throat, and let her at the fame time pronounce the Letter *k* he will feel the remaining Root of the Tongue rife towards the

the Roof of her Mouth, in order to perform it; however, she cannot keep it there any longer than the Moment of thrusting it up, for want of the Ligament (which was destroy'd with the Tongue) that is destin'd, together with the following Muscles, to keep the whole Tongue forwards in its due Situation.

The *Genioglossi* are a Pair of Muscles which arise from the fore Part of the Inside of the lower Jaw, and are inserted into the Body of the Tongue by three different Directions; the anterior Part is carried forward towards the *Apex*: the posterior runs obliquely backwards, towards the Root, sending a narrow Slip on each Side to the *Cornua* of the *Os Hyoides*; and the middle Part ends about the middle of the Tongue.

Now there are certain *Inequalities* appearing on, and closely adhering to the Floor of the Cavity of the Mouth, one of which being the most considerable, and having a Resemblance in its Substance to that of the Surface of the Tongue, has been, if I am rightly informed, inadvertently mistaken for a Tongue, by a Gentleman professing Surgery in the Country; and which he thought, for want of a careful Examination, perform'd the Offices proper to the *Apex*; but a little Care and Circumspection would have inform'd him, that those Appearances are only Fragments of the *Genioglossi* mention'd before, and that upon the Separation of the sound Parts from those mortified, such Fragments, as had escaped, were retracted, and cicatrized down into their present State; nor is it difficult to conceive how the Root of the Tongue must of Necessity sink lower down into the Throat, by the Loss of these Muscles and the proper Ligament; which, as I have

 observed

observed before, naturally kept it higher than it could remain ever since their Destruction.

If the Mortification had reached the *Os Hyoides*, it must have reached, and destroy'd the Muscles of the *Larynx*, and then the Voice would have been destroy'd; and also those of the *Pharynx*, and then Deglutition could never have been perform'd; the dreadful Consequences of which need not be enumerated here; but she swallows well, and her Voice is perfect, and therefore it is not very extraordinary she should command her Voice by the proper Muscles which remain untouch'd.

The nasal Opening is quite exposed, because the *Uvula* which cover'd it was also destroy'd; for one Pair of its Muscles (the *Glosso-Staphilini*) arise from the Tongue; by which no doubt the Distemper was communicated to this Part also.

She has her Taste perfectly, which is hereafter accounted for.

Some Considerations on the natural State and Uses of the Tongue.

THE Tongue is a fleshy Substance, chiefly made up of Muscles; and consists of a *Basis* or Root, a *Body*, and an *Apex*; the Basis is the thickest and most substantial Part, contains the *Os Hyoides*, and is naturally situated very low in the Throat: From which the Body rises upwards and forwards, and is terminated by the anterior Part or *Apex*; proceeding under the *Uvula* and Roof, and lying upon the Floor (if I may so call it) of the Mouth. As to the more particular Description of all its other Parts, I [illegible] prehend it is at all necessary here, since it is [illegible], and would take up too much of your Time.

As

As to its Uses, it is said to be the Instrument of *Speaking* and *Tasting*; as to the latter, Experience shews us that the very *Apex* of the Tongue is less capable of discerning Tastes than the next Part to it, and this than the Parts yet farther back, all along the *Body* to the Root; so that altho' the Taste of any thing is first perceived by the *Apex*, yet the Gust increases, the more the Morsel approaches to Deglutition, until it is quite protruded into the *Gula*; because as the Tongue grows more thick backwards it contains more of the nervous *Papillæ* than the smaller Part, and also because there is a Capacity of tasting in the Membranes of the back Part of the Roof to the Root; as if Nature intended to increase the Gust, that Deglutition may be the better and more eagerly performed for the Service of the Animal: Hence altho' the *Apex* and *Body* of the Tongue be gone, yet there is not a Depravation of Taste, which is the Case of the Person now under your Consideration.

As to *Speech*, which is only *Sound* or *Voice* articulated into Expression, the Tongue is not the *sole* Organ for such Articulation; the *Lips*, *Teeth*, and *Roof* of the Mouth are Instruments also for the same Purpose; the two latter for the necessary Resistance to the *Apex* of the Tongue, and the Lips for the absolute Articulation and Pronunciation of many Letters; however the following short Examination of the Letters of the Alphabet, as expressed by these Organs, will demonstrate it.

The Tongue expresses some Letters with its *Apex*, and some with its Root.

Those absolutely proper to the *Apex* are only five, *d*, *l*, *n*, *r*, *t*.

 And

And those to which it only assists are the following Letters, as *c*, *g*, *s*, *x*, *z*; all which can be performed by the Teeth alone, and which this Person does very well.

Now the Lip-Letters, and those expressed by the Root of the Tongue, she also performs as well as any Person; the former are *b*, *f*, *m*, *p*; and the latter are *k*, *q*, *x*; and as to the Vowels, and the Aspiration *h*, since they are chiefly sounded by the Exhalation of the Voice, commanded partly by the Lips in widening or straitening the Capacity of the Mouth, these she can also express; so that there is no Letter she cannot pronounce but the five *Apex* Letters; and those she manages so well by bringing the under Lip to her upper Teeth, in the Course of her Conversation, that any one can instantly apprehend every Word she says; and she further plainly proves the Lips are a better *Succedaneum* to the *Apex*, than that could be to the Lips if they were wanting.

Indeed it is natural enough for those who make the Tongue the absolute and sole Instrument of Speech, to imagine it as absurd to say a Woman spoke without a Tongue, as that she saw without an Eye; but when we consider the provisional assisting Organs ordain'd by the wise AUTHOR of Providence, serving to this necessary and expressive Accomplishment of Speech, it will not seem so extremely marvellous, that she speaks without the *Body* and *Apex* of her Tongue, as to create any further Doubt of the Matter. I am, *Gentlemen*,

[illegible] 17.

Your most humble Servant,

James Parsons.

AN

AN

APPENDIX

To the FORTY-FOURTH VOLUME

OF THE

Philosophical Transactions,

Containing some Papers, which were not ready to be inserted in the Order of their Date.

I. *Observations upon several species of small water insects of the* Polypus *kind, communicated in a letter to the* President, *from Mr.* Abraham Trembley *F. R. S.*

Translated from the French.

Read May 21, *and* June 18, 1747.

I HAVE, in a Paper printed in the 474th Number of the *Philosophical Transactions*, Art. X. taken notice of several species of small water-insects, all which have been ranged in the general class of the *Polypi*. It was during

during the ſummer of the year 1744, that the ſeveral obſervations related in that paper were made, and what reſults from thoſe obſervations, concerning the figure of theſe little animals, and their manner of multiplying, is ſufficient, if I am not greatly miſtaken, to convince any obſerver, that they well deſerve the attention of the curious. I have accordingly ſince, miſſed no Opportunity of purſuing my enquiries concerning theſe inſects, and whilſt I have been ſearching for them in different waters, I have occaſionally diſcover'd ſeveral other ſorts, to which I was before a ſtranger; but to which I have not been able to refuſe ſome ſhare of my application alſo. The relations I found theſe new ſpecies of *Polypi* had to thoſe I was already acquainted with, and ſeveral particulars in which they greatly differed, equally determined me to obſerve them all, with as much care as I was able.

This undertaking by degrees became both difficult and extenſive, yet as I was well perſuaded, that if it could be ſufficiently purſued, it would greatly contribute, both to rectifie and to enlarge our notions of Nature; I have often regretted both the want of leiſure, and the want of aſſiſtance for carrying on the work. By aſſiſtance I mean the advantages I might have received from the diligence, from the abilitys, and from the judgment, of ſuch other obſervers, as ſhould have been willing to joyn with me in ſuch an undertaking.

The more minute the objects are, upon which we are to make obſervations, the more diffident ſhould we be of thoſe obſervations themſelves. It is in theſe caſes not ſufficient to repeat ſuch obſervations ſeveral times

times by one's ſelf, but it is very proper, and frequently neceſſary that others ſhould repeat them alſo, and ſhould even ſometimes hit upon the ſame themſelves. And then it is, that by the means of theſe different obſervations carefully compared with each other, we may come to be better aſſured of the various facts we are enquiring about. Beſides which it may be noted, that the number of objects, that ſhould in this caſe be attended to, is really too great to be ſufficiently obſerved by any one ſingle perſon.

I believe it will not here be improper, to give the deſcription of that *apparatus* I have made uſe of myſelf, and by which I have been enabled to make my experiments. This deſcription will render what I have further to ſay the more intelligible; and will perhaps contribute to make others more capable of judging, what degree of credit ſhould be given to the ſeveral facts, I may have occaſion to relate. Thoſe alſo who ſhall be willing to obſerve regularly themſelves, either the ſmall water-inſects mention'd in this paper, or any others they may happen themſelves to meet with, will not, I am perſuaded, be diſpleaſed with the deſcription of an *apparatus*, which they will frequently find ſerviceable to them in their enquiries.

The principal benefit I received from it myſelf was, that I was thereby enabled to obſerve with the ſeveral magnifiers of my microſcope, ſmall water Inſects, whilſt in a glaſs containing water ſufficient to let them live therein, much in the ſame way as they would have done, had they ſtill been in the ditches or other waters from whence they were firſt taken.

If

If one only propoſes to one's ſelf, to examine for ſome moments the figures and the motions of water-inſects, one may content one's ſelf barely with expoſing ſuch in the common way to the microſcope in a few drops of water. But I dare ſafely aſſure, from divers repeated experiments, that it will often happen, with regard to ſeveral ſorts of theſe inſects, that the ſimple obſervation of them in a drop or two of water, will not be ſufficient to diſcover all that is ſingular either in their ſhape or motion. It is therefore very proper than an obſerver ſhould endeavour to examine ſuch inſects, when they are more at eaſe, and in a larger quantity of water. And this he will find ſtill more neceſſary, if he is deſirous regularly to purſue their hiſtory. For then the ſame inſects ought to be regularly obſerved, for many days ſucceſſively, and they ought themſelves alſo to be as nearly as poſſible under the ſame circumſtances they would have been, had they remained in the ſame waters, in which they naturally live.

I have accuſtomed myſelf to keep great numbers of the ſmall inſects I make obſervations upon, in large glaſſes: and it is by obſerving what paſſes in thoſe glaſſes that I endeavour to diſcover the more general facts, relating to the natural hiſtory of theſe animals. After which, I have found by many repeated experiments, that it is neceſſary to remove into glaſſes of a leſſer ſize, like that repreſented in *Fig.* 1. ſuch of the inſects as are to be ſet apart for more particular and curious microſpical obſervations. I put water into theſe glaſſes, from the ſame ditches, out of which the inſects I am obſerving have themſelves

ſelves been taken; and I ſhift this water more or leſs often as the circumſtances may require.

It is eaſy to conceive, that to obſerve a ſmall inſect in one of theſe glaſſes, with a magnifier of a ſhort *focus*, it is neceſſary that inſect ſhould be placed very near to one of the ſides of the glaſs; and that it ought alſo to be kept ſteadily in the ſame place. The inſect ought therefore to be either fixed to the ſide of the glaſs itſelf, or to ſome other body that may be conveniently ſo fixed. I chuſe, for this purpoſe, ſubſtances that are ſlender and ſupple, ſuch for example as the ſmall branches or twigs of divers ſpecies of the *Equiſetum paluſtre*, or water horſetail. The cluſtering *Polypi* are often found upon theſe twigs, and they may be made to ſettle upon them from elſewhere, as I ſhall take notice, by and by.

Now this is the way I take, to fix one of theſe twigs of horſetail againſt the ſide of my glaſs. Having choſen a ſmall ſlip, upon which there is one or more of the cluſtering *Polypi* or the like, I take a piece of a peacock's feather, longer or ſhorter, according to the diameter of the glaſs I am then to uſe. From this piece of peacock's feather I cut away all the lateral branches or beards on both ſides, excepting one at one of its extremitys; upon this one I make a knot near its inſertion, but do not at firſt draw it cloſe. I then bring this open knot to the ſmall ſlip of the horſetail that is floating in the water of the glaſs, and I get one of its extremities into the knot, which I then draw cloſe; and the ſlip of the horſetail is thus joyned to the piece of the feather. I next take hold of the feather, and bending it near the middle, I force its two ends (Tab. I. *Fig.* 4.) *b*,*f*, into the

Nnnn glaſs

glaſs, I then let go the feather which I before held, and its elaſticity forces its two ends againſt the ſides of the glaſs *A*, by which means the ſmall twig *dl* of the horſetail I have been ſpeaking of, and which was already fixed to the extremity of the feather *fd*, becomes alſo fixed cloſe to the ſide of the glaſs; the conſequence of which is, that the *Polypus* that is ſticking to the horſetail is obliged to remain in ſuch a ſituation, as to be within the reach of a magnifier that is but of a ſhort *focus*.

Nothing more is now wanting, but to place the magnifier before the object: for it would be both difficult and very inconvenient to hold it like a reading-glaſs in the hand. In the inſtrument *k*, *i*, *h*, *g*, *e*, I make uſe of, it is ſcrewed into a ring fixed to a ſmall branch *n*, *g*, which has a ball *g* at its other extremity; this ball fits a ſocket, and ſo makes a joynt, by which the firſt branch is joyned to a ſecond *h*, *i*, and that again in like manner to a third *i*, *k*, or fourth, if there is occaſion. The foot of the whole is fitted near the edge, into a ſmall board or tablet that holds the whole *apparatus* (*Fig.* 4.). By the means of theſe joynts, the magnifier *e*, may be turned any way, and may be conveniently brought near to its proper diſtance from the object, yet as the branch which holds it, cannot well be without ſome ſpring; it will be ſtill difficult to adjuſt the object exactly to the *focus* of the magnifier when it is ſhort, if only the magnifier was to be moved for that purpoſe; and it will therefore be found eaſier, when the magnifier is once right againſt the object, to move gently the glaſs in which that is contained, till it is found to be preciſely in the *focus* of the magnifier:

magnifier: and for this purpoſe the ſmall board upon which the glaſs is placed, ought to be well ſmoothed.

The light that comes in at a common window will be found ſufficient, for obſerving in the water ſuch objects as are to be ſeen with the bare eye, or with a hand magnifying glaſs; but ſuch as muſt be examin'd with a *lens* of a ſhorter *focus*, muſt be view'd by the light of a taper, placed beyond the glaſs, and whoſe flame is ſo order'd as to be upon the level with the object.

A magnifier thus once adjuſted may remain in the ſame place before the object, for ſeveral days together, without being diſorder'd; ſo that, to obſerve the progreſs of the inſect during all that interval; no more will be neceſſary, than to place from time to time a taper behind the glaſs, and to apply the eye to the already fixed magnifier.

Several of theſe *apparatus's* may be placed upon one and the ſame board by one another; and thus at the ſame time obſervations may be made and carried on upon different ſorts of inſects, or upon ſeveral inſects of the ſame ſpecies; in order to come ſooner and with more certainty at the knowledge of the facts one is enquiring about.

could never have diſcovered the manner in which the cluſtering *Polypi* are multiplied, but by the help of the expedient I have juſt deſcribed: and before I had the uſe of that *apparatus*, I only knew in general the figures of thoſe *Polypi*, and of the cluſters that contained them. I had taken notice that thoſe cluſters grew, and I had reaſon to ſuſpect, that a whole cluſter came from a ſingle *Polypus*;

but I ſtill wanted to ſee this increaſe, and to find the moment of their multiplication; for I had reaſon already to ſuſpect, from what I had ſeen with a glaſs I held in my hand, that theſe cluſters did not grow inſenſibly like plants; but that on the contrary, the operation I wanted to ſee was performed in a ſhort portion of time. To come therefore at that moment, I reſolved to obſerve regularly for ſome time *Polypi* of this ſort with my Microſcope, whilſt they ſhould remain in circumſtances, nearly as eaſy and as natural to them, as thoſe they were in in their proper habitation.

This it was that gave me the firſt thought of the above-deſcribed *apparatus*. And when I had prepared and fixed every thing, I ſet myſelf continually to watch for the moment of the multiplication of the cluſtering *Polypi*; and I then found this moment, which I had ſo much wiſhed to diſcover, the very ſame morning that I began to make uſe of my *apparatus*.

It was, as has been ſeen in the paper above referred to, in that ſpecies of *Polypi*, ſome of which are repreſented in the 5th, 6th, and 7th figures of the 2d plate of the 474th number of the *Philoſophical Tranſactions*, that I firſt diſcover'd the manner in which theſe ſmall animals are multiplied: and it is indeed among ſeveral ſpecies that I am now acquainted with, one of thoſe in which this fact is the moſt eaſy to be obſerved.

It is alſo in the ſame ſpecies eaſy to ſee that very odd motion, which they exhibit at their anterior extremity.

This ſame motion, which has alſo place in other ſpecies of cluſtering *Polypi*, is not in them ſo eaſie

to

to be remarked; both on account that they are lesser, and also on account that this motion itself is swifter, than in the sort above-mention'd.

There is also to be observed at the anterior extremity of several other small insects, a sort of motion which has drawn the attention of all such as have happen'd to see it, and who have almost all been curious to enquire and satisfie themselves, whether those little wheels, which appear to turn with so swift and so regular a motion, are really wheels turning upon an axis or not. This has determined me now to mention that motion, tho it is not my design to treat fully of it in this place, or to determine very precisely what I think about it: as I shall be very cautious how I assert any thing positively upon so nice a matter, until I shall have repeated again several experiments I have already made, and until I shall have tried several others.

In order to discover what this motion might really be, I have applied myself not only to observe it in the same animal placed in different altitudes, but also in different species of water-insects in which it is seen, and I have compared the *Phænomena* of all these several motions one with another. These comparisons I have found in other cases to be of singular use, and the best means of preserving myself from those illusions, which very small objects, view'd in a microscope, especially whilst they are in motion, are but too apt to present.

All I have yet learned from these comparisons, and all the other observations I have made, seem to concur in proving to me, that there is some deception of the sight in the present case, and that the

the motion in queſtion is not really what it at the firſt appears to be, a rotatory motion round an axis. And I even know ſome ſpecies of *Polypi*, in which this motion is, comparatively ſpeaking, but ſlow: and in theſe it is diſtinctly ſeen, that this motion, tho' in general reſembling that obſerved in the others, is not a revolving or rotatory motion: ſuch, for example, is the motion which is taken notice of in that ſpecies of *Polypi*, which Mr *Leewenhoeck* has deſcribed in the 295th Number of theſe *Tranſactions*. This is one of thoſe inſects whoſe motion is the moſt to be admired, and it is beſides exceedingly curious upon many other accounts.

I have already ſaid, in my paper above referred to, that the motion in queſtion is very ſlow in the cluſtering *Polypi*, juſt when they are opening again after their diviſion, and I am greatly miſtaken, if it may not then be ſeen very plainly that this motion is not a rotation. The ſame remark may alſo be made on the tunnel-like *Polypus*, and that, almoſt during all the time that it employs in its ſeparation.

I made uſe of an expedient, whilſt I was obſerving the cluſtering *Polypi*, whereby I was able to retard the quickneſs of their motion. I poured by little and little a ſmall quantity of ſpirit of wine into the glaſs wherein they were kept. This ſpirit of wine immediately either abated the velocity of their motion, or took it quite away, according to the quantity of it that I poured in. That which follows both in the one and in the other of theſe two caſes, is of uſe, and gives light to the preſent queſtion. Sometimes the ſpirit of wine forces the

Polypus

Polypus entirely to draw in its lips within its body, and at other times even to detach itſelf entirely from its pedicle alſo.

Another way to take off the celerity of this motion, is to remove the inſects into a water which furniſhes them much more ſparingly with food; faſting probably weakens them, and from their weakneſs ariſes an abatement in the quickneſs of their motions. This laſt expedient is of uſe and conveniency for the obſerving of this motion whilſt it is ſlower, for ſeveral days conſecutively. And afterwards upon returning the *Polypi* into water ſtocked with food for them, the motion will ſoon be reſtored to its former briskneſs.

I remarked alſo the laſt winter, that cold deadened the motion of the cluſtering *Polypi:* and theſe animals in all probability are leſs voracious, and eat leſs in winter than they do in ſummer.

When the motion in the cluſtering *Polypi* has been retarded, either by faſting or by the cold, they become whiter or of a paler colour than before, they alſo then ceaſe to multiply.

I ſhall not here enter into the detail of the ſeveral obſervations I have made, on the feeding of theſe cluſtering *Polypi,* and on the relation I have found between that and their generation; as theſe are particulars more properly belonging to a regular and diſtinct account of their natural hiſtory.

But what I now propoſe, is to deſcribe, in a few words, the manner in which the cluſters are formed of a certain ſpecies of *Polypi,* which multiply in the main like thoſe repreſented in the figures of the 474th number of the *Philoſophical Tranſactions,* and

and which differ chiefly from them in the form of their clusters.

My chief end in describing here this species of *Polypi*, is to enable myself afterwards, to convey, by comparison, a distinct idea of a difference well deserving attention, that is to be observed between the manner of multiplying these, and that of another species of clustering *Polypi*, which I happened to discover the last year.

The reader will please to recollect what I have said in the forecited paper, concerning the general manner in which clustering *Polypi* are multiplied. These little animals are nearly of a bell-like form. Their anterior extremity, in which is their mouth, and which may be looked upon as their head, is that which is hollowed inwards, and resembles the open end of the bell. Their other extremity terminates in a point, and to this point is fixed a stalk or pedicle.

The *Polypus*, when it is ready to divide, first draws in its lips into the body. It then by degrees puts on a round form, and presently after the little spherical body so formed, divides itself into two other like spherical bodies. These last in a few moments again insensibly open, they then lose their spherical form, and put on that of a bell, or of a *Polypus* as perfect and as compleat, as that by the division of which it was formed. This is the manner in which several species which I have observed of clustering *Polypi* are multiplied: the whole operation is performed by that sort, of which I have spoken in my former paper, in three quarters

quarters of an hour or an hour by thoſe I am now going to ſpeak of.

The *Polypi* of this ſort are leſſer and whiter than thoſe others, which are repreſented greatly magnified in the above-mentioned figures. The cluſter which they form reſts upon a ſtem eaſie to be remarked: this ſtem is fixed to ſome other body at its lower extremity, and from its other ariſe branches, making obtuſe angles with the ſtem itſelf; other branches again ſet out from theſe in different places, and from theſe laſt other new ones, and ſo on. At the extremity of each branch may be ſeen a *Polypus*: and as all theſe branches are not of an equal length, ſo neither is every *Polypus*, as in the other ſpecies, at the top of the cluſter, or at an equal diſtance from the baſe of the ſtem, but on the contrary, there are here *Polypi* to be diſcovered at all heights in the cluſter. The aſſemblage of all theſe branches forms, together with the *Polypi* at their extremitys, a very pretty cluſter or groupe, much reſembling a tuft or a garland of flowers.

The ſtem, which carries all the cluſter, and every branch in it, is capable of a remarkable ſort of motion. Each will contract ſuddenly when it is touched, when the glaſs containing the cluſter is moved, and even ſometimes when no reaſon is to be perceived for their ſo contracting (*Fig.* 6. *a*). The ſtem and the branches contract and ſhorten, by diſpoſing themſelves into ſpirals, all whoſe rings nearly touch each other. Every branch is by itſelf capable of contraction, independently of the reſt: tho it but rarely happens that any one branch does contract itſelf quite alone, for commonly in the action of con-

tracting it happens to touch ſome other branch, and then that other immediately contracts with it. When the main ſtem, which bears the whole cluſter contracts itſelf, then all the branches of the cluſter con tract together alſo; and the whole becomes entirely cloſed. A moment after, the branches and the ſtem again extend themſelves, and the whole cluſter thereby recovers its ordinary figure. But when the cluſter is conſiderably advanced, the ſtem then ceaſes to contract itſelf any more.

I ſhall now attempt to deſcribe the manner in which this cluſter forms itſelf.

A ſingle *Polypus* detaching from the cluſter, ſwims about in the water till it meets with ſome proper body to fix itſelf upon. It then *has* a pedicle but which is not longer than the *Polypus* itſelf. In the ſpace of 24 hours this ſtem becomes 8 or 9 times as long as it was at the firſt: and it is this pedicle which is to become the main ſtem of the new cluſter. About a day after the *Polypus* has been thus fixed, it divides itſelf into two. Ten or twelve hours after, theſe two *Polypi* again divide themſelves each into two more: they ſoon after put out branches, and thus retire to a greater diſtance from each other. It is now neceſſary to take notice, that when two of theſe *Polypi* are thus formed by the diviſion of one, the one is ordinarily much larger than the other: this larger one remains at the extremity of the branch where it was, but which branch lengthens it ſelf more, whilſt the other puts out a new branch which ſeems to proceed from the firſt. The larger of theſe *Polypi* again divides it ſelf generally before the other; and all I

have

have been describing is reiterated ſeveral times. Thus a principal branch is formed, provided with ſeveral lateral ones. Theſe lateral branches become principal, with regard to thoſe which in their turn ſeem to ſpring from them, when the *Polypi* at their extremitys come to divide. All the *Polypi* of a cluſter do not detach themſelves from it at the ſame time: thoſe which are neareſt to the origin of the branches uſually detach themſelves firſt. And every *Polypus* ſo detached, goes and fixes itſelf elſewhere, every one thus becoming at laſt, if not prevented, the principal of a new cluſter.

I have often kept *Polypi* of this ſort, in glaſſes of the ſize of that which is repreſented in *Fig*. 4. And the firſt cluſter I had placed in it to obſerve its growth and progreſs, continued ſtill well provided with *Polypi*, when there were already numbers of other cluſters formed in the ſame glaſs, all which owed their being to thoſe that had detached themſelves from the firſt cluſter. I have ſeen ſometimes, portions of the peacock's feather in the water, entirely covered with theſe cluſters: and I was well aſſured that all theſe cluſters came from the firſt I had lodged in the glaſs. Nay I have even carried my experiments ſo far as to be well aſſured, that every *Polypus* of a cluſter, as ſoon as detached and fixed elſewhere, became the principal of a new cluſter. I mention this fact particularly, becauſe I ſhall make ſome uſe of it hereafter, when I come to take notice of a difference, between this ſpecies of *Polypi* I am now treating of, and another ſpecies I ſhall have occaſion to ſay ſomewhat about by and by.

 Wher

When a cluster is already in good part stripped of its *Polypi*, the branches are no longer able to contract with the same quickness and readyness as before. When there remain but a very few *Polypi*, none but those branches to which *Polypi* are still fixed continue to exert this power; which *they* also lose as soon as they are stripped of their few remaining *Polypi*, after which they shew no further capacity of moving.

From all which particulars it seems to result, that this motion in the stem and in the branches of a cluster, is entirely derived from the *Polypi*, which are fixed upon the branches. Notwithstanding which it must be acknowledged, that an observer, attending to the appearance only of this motion, can hardly help persuading himself at the first, that they are the branches, which draw and give motion to the *Polypi*.

The resemblance and the analogy, which the figure of a cluster of *Polypi* bears to the figure of a plant, would induce any observer, for some time to imagine, that the *Polypi* which he sees fixed to the branches of the cluster, do really proceed and spring from those branches, in the same manner as the leaves, the flowers, and the fruits of a vegetable, spring from the branches of the same.

It is nevertheless the contrary of all this, that is true. The branches, composing the clusters of the *Polypi*, spring from the *Polypi* which are at their extremities. These *Polypi*, which at the first appear to be the fruits of the clusters, may more properly be considered as their roots: and of the truth of this any one may easily satisfy himself, who will be

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be at the trouble of examining regularly, and for ſome continuance, the whole progreſs of a cluſter of theſe *Polypi*.

What further proves that theſe branches do really ſpring from the *Polypi*, and that they derive their nouriſhment from the ſame, is, that the branches conſtantly ceaſe to grow, whenever the *Polypi* at their extremities are detached from them, either naturally or by any accident.

The *Polypi* of another ſpecies I am now going to ſpeak about, form alſo a groupe reſembling a cluſter, or more properly an open flower. This flower or cluſter is ſupported by a very diſtinct ſtem, which is by its lower extremity fixed to ſome of the aquatic plants or extraneous bodies that are found in the water. From the other extremity of this ſtem ſet out eight or nine branches, quite differently diſpoſed from thoſe of that ſpecies of *Polypi* I have been laſt deſcribing. Theſe eight or nine branches are perfectly alike, but it may be noted, that what I here call by the name of a branch, is indeed the aſſemblage of ſeveral other leſſer branches, whoſe collective form much reſembles that of a leaf. (*Fig.* 4.). Every one of theſe aſſemblages is compoſed of one principal branch or nerve, which makes with the main ſtem of the cluſter an angle ſomewhat greater than a right one. From either ſide of this principal nerve others again ſet out, and theſe lateral ones are the leſs extended in length, the nearer their origin is to the extremity of their principal branch. There is a *Polypus* at the extremity of this principal branch, and another at the extremity of every one of the lateral twigs.

There

There are others also on both ſides of thoſe lateral twigs, at different diſtances from their extremities, and theſe are more in number or fewer, in ſome proportion to the length of the twig itſelf. Theſe *Polypi* are all exceedingly ſmall, and of a bell-like figure, and they diſcover about their openings a quick motion, very difficult to be ſeen with any diſtinctneſs.

There may alſo be obſerved in ſeveral places, upon the branches of theſe cluſters of *Polypi* (*Fig.* 7.) certain round bodies, which I at the firſt took for inſects preying upon the *Polypi*, becauſe I was acquainted with ſome ſuch, nearly of that ſhape and ſize: but I ſhall preſently give an account of what thoſe round bodies really are.

Every cluſter has, as I have ſaid, eight or nine of theſe branches or leaves ſuch as I have juſt deſcribed. They do not all of them ſet out from the ſame point; but the points from whence they do ſet out are not far aſunder: each of theſe leaves is a little bent inwards, and they all form together a ſort of a ſhallow chalice or cup. If the eye is placed right over the baſis of this chalice, the appearance of the whole eight or nine branches is like unto that of a ſtar with ſo many rays proceeding from the ſame center.

When the cluſter is touched, and even frequently without it, all the branches fold together inwards, and then conſtitute a ſmall round maſs. The ſtem, which carries all the cluſter, contracts alſo at the ſame time, folding it ſelf up like a workman's meaſuring rule, that conſiſts of three or four different joynts.

I ſaw

I ſaw for the firſt time the *Polypi* I have now been deſcribing, on the 30th day of *May* of the laſt year 1746. They were upon a water-plant, which I had taken from a ditch, and diſpoſed in one of my large glaſſes. They immediately ſtruck me by their beauty, and I could not help being curious to know, in what manner ſuch cluſters were formed. The relation they bore to the ſpecies firſt above deſcribed, and to ſome other ſpecies which I had before obſerved, gave me reaſon to believe that the cluſter muſt have ſprung from a ſingle *Polypus*, by the means of ſeveral ſucceſſive diviſions. I was not however contented with judging of them from analogy only; I was deſirous to be actually an eye-witneſs of their operations; and the obſervations which I therefore made upon them, diſcover'd to me a new fact, which I ſhould never have ſuſpected, and which I could never have come to the knowledg of, if I had contented my ſelf with the judgment I made of them from Analogy only.

I ſuppoſed, when I began to obſerve, that every cluſter in queſtion came from a ſingle ſmall *Polypus*, like to thoſe with which the cluſters were ſo plentifully provided. I therefore began by endeavouring to get one of theſe *Polypi* ſingle, and fixed upon ſuch a body as I could well diſpoſe in my glaſs, ſo as to keep it within the reach of a magnifier of a ſhort *focus*; and I purſued for this purpoſe my ordinary method.

I took ſome cluſters of theſe *Polypi* well advanced, I put them apart in a glaſs filled with proper water to afford them ſuſtenance; I put alſo into the ſame glaſs a ſlip of water-horſetail, after I had carefully

carefully examined it, and so assured myself that there was no *Polypus* upon it. I expected that some *Polypi* would soon detach themselves from the clusters, and that some of those *Polypi* would fix upon the horsetail, whereby I should be enabled to set them apart, and to observe in other glasses the progress of the clusters, which would, as I made no doubt, be soon produced from them.

It was on the 30th of *May*, that I set the clusters apart in the glass; on the 31st I could discover nothing new, and on the 1st of *June* I had no opportunity of observing; but on the 2d in the morning I found against the sides of the glass several small clusters of *Polypi*, of the species I am now treating of. I was surprized to find them so far advanced, for they could not have begun at the soonest before ten a clock at night, on the 30th of *May*. I saw on the 2d of *June* in the afternoon upon the slip of the horsetail, which I had placed in the same glass with the clusters of the *Polypi*, a small body, which, as I had all reason to believe was newly fixed upon it. I then took out the slip of the horsetail, and I lodged it with the small body that was upon it in another glass; after which I examined that small body with my microscope, by the help of the *apparatus* first above described.

I then found that this body was much larger than any of the *Polypi* of the present sort, and of a figure very different from them (*Fig.* 8.) This made me suppose that this body was not of the species of the *Polypi* now before us, and that it was not from any thing of this sort that I was to expect the production of a cluster of this

this species of *Polypi*. I resolved however to continue my observations upon this minute body; which was oblong, and had a pedicle three or four times longer than it self.

It was on the second of *June* at 5 in the evening that I put it apart in a glass, and at half an hour after 8 the same evening, I perceived that it began to split from the top towards the bottom. When the separation was accomplished, each of the two bodies, formed by this division, was nearly of the same shape as the first (*Fig.* 6.). I then thought, judging still by analogy, that it would be some time before either of these bodies would again be ready to divide; but a very little after, I saw that they both became round, and that they disposed themselves precisely as if they were again going to separate. This novelty drew all my attention, and it again came into my mind, that this body which I had but just concluded not to contain the principle, from whence I was to expect the production of one of the clusters I was looking after, might possibly still be the very thing I was seeking for.

I now imagined that perhaps these bodys would again divide and subdivide themselves, till they should come both to the shape and to the size of the *Polypi*, which I had seen upon the clusters: I however looked upon this Idea but as a mere conjecture. The two little bodies did in effect divide presently after; but the 4 which resulted from this division (*Fig.* 7.) had neither yet the form nor the minuteness of the *Polypi* in question. I now wanted to know whether these 4 bodies would again proceed to divide without interruption; and I saw them a

 little

little after again prepare for another diviſion : this diviſion was completed at 20 minutes after eleven, and at midnight the 8 bodies which were formed by this third diviſion were again almoſt compleatly divided. The cluſter was then compoſed of 16 *Polypi*; and I from that moment no longer doubted, but theſe were cluſtering *Polypi* of the ſpecies I have been laſt deſcribing. Among theſe 16 *Polypi*, there were ſome which had already the perfect form of thoſe I had obſerved upon the more advanced cluſters: and theſe were ſuch as were neareſt to the origin of the branches.

Few of theſe 16 *Polypi* were of an equal ſize, thoſe which were the moſt diſtant from the origin of the branches, were the largeſt, and their form alſo was the leaſt like to that of a bell. I found at three in the morning on the third of *June*, that the number of the *Polypi* in the cluſter was conſiderably encreaſed; they were 16 at midnight, and I could now tell 26, tho I could only ſee part of the cluſter, the reſt of it being beyond the *focus* of the microſcope: and at half an hour after 7 in the morning, I counted at leaſt 40 *Polypi*, in that ſame part which I could ſee of the cluſter.

In order to judge with more certainty of the progreſs of the multiplication of theſe *Polypi*; I counted alſo thoſe of another cluſter, which was ſo ſituated as to be entirely within the reach of one of my magnifyers. This cluſter began to be formed about 8 in the evening of the 2d of *June*; I mean that it was then, that the round body firſt began to ſplit it ſelf into two. At 11 the ſame night, that cluſter conſiſted of 8 *Polypi*, at half an hour after 7 the next

next morning of 64, and before night of 110 at the leaſt. So that in about 24 hours there were formed, by repeated diviſions of one ſingle round body, no fewer than 110 *Polypi*.

The cluſter I firſt ſpoke of continued to encreaſe from the 2d of *June* at half an hour after 8 at night, when it firſt began to form it ſelf, till the 13th, when the *Polypi* began to detach themſelves from it; and there remained no more upon the cluſter on the 15th.

The *Polypi* which are at the extremitys of the principal branches are conſtantly the largeſt, they are thoſe which divide themſelves the moſt frequently, and one of the 2 *Polypi* reſulting from this diviſion is generally larger than the other. The largeſt remains at the end of the principal branch, whilſt the leſſer ſerves to form a lateral branch, and is it ſelf the principal of all the *Polypi* which that lateral branch is to bear.

One can hardly now be without curioſity to know, what thoſe round bodies really are; thoſe ſort of bulbs which contain in themſelves the principle, from whence theſe whole cluſters we are ſpeaking of are to be produced. What gives origin to theſe bulbous bodies? Are they produced in the cluſters by diviſions and ſubdiviſions, as the *Polypi* themſelves are, which in other ſpecies are themſelves the principles of the cluſters? In theſe other ſpecies, every *Polypus* may become the principle of a cluſter and of a groupe of *Polypi*, as ſoon as it has detached it ſelf from the cluſter where it had its origin. When one of theſe has once fixed alone any where and divided it ſelf, it no ways differs either in ſhape

 or

or in ſize, from any of the *Polypi* that were in the cluſter it is now parted from, or from any of thoſe others that will be formed in the cluſter, it is by its own future diviſion and ſubdiviſions to produce. But how is it with the new ſpecies we are now conſidering? Does every *Polypus* among theſe, as ſoon as detached from the cluſter, fix it ſelf alſo elſewhere, and there give origin to a new cluſter? Or are they only the bulbous bodies above mentioned, that have this prerogative, of being capable to produce a new colony?

Theſe queſtions and doubts greatly raiſed my curioſity, from the time I firſt began to ſee the progreſs of a cluſter of *Polypi*, formed by the diviſion and the ſubdiviſions of one of theſe round bulbous ſubſtances: and that which now follows, is what I have been able to collect from the various obſervations, and from the ſeveral experiments, which I made, whilſt I was endeavouring to give my ſelf ſome ſatisfaction with relation to the ſame doubts and queſtions.

To know, whether the *Polypi* which detach themſelves from theſe cluſters do each of them contain in themſelves the principles of other new cluſters, I took all the precautions I had taken in other caſes, and ſuch as I had found eaſily to ſucceed with the cluſtering *Polypi* of other ſorts. But all was to no effect, and I could never find that any thing was produced by theſe *Polypi* ſo detached. I have therefore all reaſon to preſume, that theſe *Polypi* do not contain the principles of new cluſters, and it ſeems to me the moſt probable, that they all periſh without ever producing any thing whatſoever.

When

When I first began to seek for the origin of the round bulbous bodies I have been speaking of, I immediately recollected those other round bodies I had before taken notice of, and which I at the first suspected to be insects preying upon these *Polypi.* I therefore again sought for them in the clusters already formed; I soon found several of them, and I perceived that they neither attacked the *Polypi* nor changed their situation. I then concluded that these round bodies were really the very bulbous ones in question, and whose origin I was now seeking for: I applied my self therefore to observe several of them, and these are the facts which I then discovered.

Some days after the clusters had begun to form themselves, I saw come out, not from the extremities of the branches, but from the bodies of the branches themselves in different places, small round buds, which grew very fast, and which arrived at their greatest size in two or three days. These bodies much resembled the galls which grow on the leaves of oaks; they were placed upon the branches of the clusters, just as those galls are usually placed upon the fibres of the leaves: and these bulbous substances do really contain the principles of the clusters.

Two or three days after these bulbs have begun to form, they detach themselves from the branches out of which they sprung, and go away swimming till they can settle upon some body, which they meet withall in the water, and to which they immediately fix themselves by a short pedicle. The bulbs are then nearly round only a little flatted on the

under

underside, the pedicles continually lengthen themselves by degrees for about 24 hours, and during the same time the bulbs also change their figure, and become nearly oval. There are in a cluster but few of these bulbs, in comparison of the great number of *Polypi* that are upon the same; neither do these bulbs all come out at the same time.

It is now easie to judge of the remarkable difference there is between the two sorts of clustering *Polypi* that are described in this paper.

The clusters of the first species of *Polypi*, and those of several others which I have also observed, do all come from *Polypi* detached from the clusters already formed. But the clusters of the *Polypi* of the second species here described, do not arise from *Polypi* detached from other clusters, but from round bodies or bulbs, larger than those *Polypi*, and of a form very different from them.

These bulbous bodies are not formed like the *Polypi*, by the division of others like themselves, but they spring from the branches of the cluster, as the flowers and the fruits of a tree spring from the branches of the same.

In diverse other species of *Polypi*, there are considerable intervals of time between their divisions. In the bulbous kind, if I may call it so, the first divisions are consecutive and follow hard upon each other, nor is there any interval of time between them, until the bodies which are to divide have already acquired the shapes of *Polypi*.

The clusters of the bulbous sort have an origin entirely different from those of the other sorts of clustering *Polypi*. Yet do these clusters enlarge, and the

the *Polypi* upon them multiply, in the ſame manner as thoſe of the other ſpecies which I am acquainted with.

As I relate facts that are new, and as I am alſo, if I may ſo ſpeak, under the neceſſity of mentioning new relations and analogies, I find my ſelf under great difficulties, to find proper terms to expreſs thoſe relations and analogies.

I ſhall not here enlarge upon the analogies which may be found, between the origin of the minute animals I have been ſpeaking of, the origin of plants, and the production of thoſe other animals we have been hitherto more acquainted with. We ſhall better be able to judge of thoſe analogies, and to compare them together, when we ſhall come to know more both of plants and of animals, and when we ſhall have made obſervations upon greater numbers of them.

The new and the ſurprizing facts, which the ſtudy of natural hiſtory lays before us more and more every day, are fully ſufficient to convince us, that the nature both of plants and animals is as yet but very imperfectly known to us, and indeed much more imperfectly than many have been apt to imagine. All we do know is but very little, in compaparifon of what yet remains to be known: and this conſideration ſhould prompt us, ſtill more aſſiduouſly and more diligently, to enquire after truth; as it ſhould at the ſame time alſo make us exceedingly circumſpect, and very cautious how we venture to make judgments upon the nature of things, or how we form to our ſelves general rules, from ſo few principles as we are at preſent maſters of.

Explanation

Explanation of the figures *in* TAB. I. *referred to, in the foregoing Paper.*

The 4th figure in TAB. I. represents the necessary *apparatus*, for observing commodiously and regularly a *clustering Polypus* with the microscope. In the glass *A*, is the end of a peacock's feather *b*, *c*, *f*, bent at *c*, and whose extremities are by the spring of the feather, kept close against the sides of the glass. At one of the ends *f* of the feather one of its beards is left on, which is long enough to fasten to it in *m* a slip of water horsetail *dl*, upon which is a *Polypus*, which is by this means kept so close to the side of the glass, as to be within the reach of a magnifyer of a short *focus*, such as *e*. This magnifyer is screwed on to a ring whose arm *ng* has at its extremity *g* a ball playing in a socket so as to make a joynt; there are again other like joynts at *h* and *i*, and by the help of these the magnifyer may be moved every way, and be conveniently brought near to the object. The foot *ik* is stuck into the board upon which the glass is placed. The light of a window in the day-time is sufficient to observe an object so placed within the glass, either with the bare eye, or with an hand-magnifyer: but if a magnifyer of a short *focus* is necessary, the shutters must be closed, and a wax light must be placed behind the glass, at such a height as to have its light fall directly upon the object; and a magnifyer so placed may remain if there is occasion for several days in the same posture without any inconvenience.

The

The 5th figure exhibits a cluster of *Polypi*, of the first of the two species described in this paper, and which is here considerably magnified.

The 6th figure shews another cluster of *Polypi* of the same sort; the number of the *Polypi* here shewn is but small, because the cluster was drawn as it appeared within 2 or 3 days after it had first begun to form it self. One of the branches of this cluster is partly contracted, and they may be seen in this scituation, when a branch after contracting it self is again expanding to its ordinary state. This cluster is yet considerably more magnified than that exhibited in the 5th figure.

The 7th figure represents one branch of a cluster of *Polypi* of the second species described in this paper. There may be seen upon this branch, besides the *Polypi* which are of a bell-like form, some of those round bodies from which the clusters of this kind of *Polypi* do first spring; and which remarkably distinguish it from many other species.

The 8th figure represents one of these round or globular bodies, after it has parted it self from the cluster, has fixed it self to some other body, and after that the globule itself and its pedicle have begun to lengthen. It was in this condition on the second day of *June* at 5 in the evening.

The 9th figure exhibits the 2 bodies, that were formed by the parting of that represented in the 8th figure. This parting began at half an hour after 8, and was completed at 9 the same evening.

The 10th figure represents the four bodies, which were formed from the 2 represented in the 9th figure; and these four bodies were also formed before 10 of the clock.

II. *A collection of the* magnetical Experiments *communicated to the* Royal Society *by* Gowin Knight *M.B. & F.R.S. in the Years* 1746 *and* 1747.

I.

An account of some magnetical Experiments, *exhibited before the* Royal Society *on* Thursday *the* 19*th of* February 1746, *and of which* the President, *who had before seen the same performed with more deliberation on the* 11*th of the same month, was pleased to make the following report.*

Read Feb. 19. 1746-7. BEING on *Wednesday* the 11th of this instant *February* at the house of Mr. *Knight*, I did there in company with our worthy brother *William Jones* Esq; see the following experiments; which Mr. *Knight* was desirous I should, as on this day, report to the *Society*: before whom he is also now prepared to exhibit the same, as well as the circumstances of the place and the number of the company will allow.

He first produced two almost equal bars of hardened steel, to which he had communicated a strong magnetic virtue. These bars were nearly square, each being of the length of about 15 inches and two tenths, and of the breadth and thickness of a little more than half an inch: one of these bars

bars weighed 2 pounds and 6 pennyweight *Troy*, the other 4 pennyweight leſs than 2 pounds; and either of them readyly lifted with one of its ends better than 3 pounds and a half.

Theſe bars were then laid down on a table, ſo as to be nearly in one and the ſame ſtrait line, the north pole of the one being next to the ſouth pole of the other, and at the diſtance of about an inch from it: that is to ſay, that the north poles of both bars were pointed the ſame way, but without any regard to the poſition of the natural meridian.

Mr. *Knight* then produced a piece of natural magnet, which was one of the ſame he had formerly made uſe of, in ſome experiments he had before ſhewed to the *Royal Society*. This piece was in length an inch and $\frac{1}{10}$, in breadth $\frac{7}{10}$, and in thickneſs about $\frac{3}{10}$ of an inch at a medium, being conſiderably thicker at the one end than at the other.

This piece of magnet was then applied, ſo as to lie between the 2 firſt mentioned bars, with its thin end cloſe to the north pole of one of them, and its thick end cloſe to the ſouth pole of the other. After it had lain in this poſition a few moments, it was taken out, and upon preſenting it to the magnetic needle of a ſmall compaſs box, it was obſerved that its thinner end, the ſame which had juſt been contiguous to the north pole of one of the bars, attracted the north end of the needle; and that the thicker end, the ſame which had been contiguous to ſouth pole of the other bar, attracted the ſouth end of the ſame needle.

This ſame piece of ſtone was then again put in between the bars, but in a contrary poſition; the thicker end now lying next to the north pole of one of

the bars, and the thinner end next to the south pole of the other. After a few moments it was again taken out, and presented as before to the compass-box: when it was found that the thinn end now attracted the south end of the magnetic needle, and that the thicker end attracted the north end of the same.

The piece of stone was then again placed between the bars as at the first, and being again taken out and presented to the compass-box: the thin end was again found as at the first to draw the north end, and the thick end to draw the south end of the needle.

This same piece of magnet was then again placed between the bars, but in a position at right angles to both the former, one of its sides being now contiguous to the north pole of one of the bars, and its other side to the south pole of the other. After which being again in a few moments taken out, and presented to the compass-box as before; it was found that the side which had been in contact with the north pole of one of the bars, did attract the north end of the needle, and that the other side which had been in contact with the south pole of the other bar, did attract the south end of the same needle: whilst the two ends of the stone in which the polarity was before observed, were now found to be indifferent to either end of the needle; so that the line of direction of the poles in the stone now lay at right angles to the position in which it was scituated in the former experiments.

Mr. *Knight* then produced two steel needles, of the same sort as those which are usually fixed to the cards of sea-compasses. These needles were of the length of 5 inches and $\frac{2}{10}$, and weighed severally with their caps 7 pennyweight eight, and 7 penny-weight

weight nine grains; one of these was tempered and of a blew colour, and the other was quite hard. He also produced two iron weights, severally weighing 14 pennyweight 22 grains, and 15 pennyweight 7 grains, both nearly of a cylindrical form, but with one of the ends rounded off.

The 2 large bars were then placed in a line, as in the former experiments, but with their ends so near together, as only to admit of the cap of one the needles between them.

The tempered needle was then placed flat upon the bars, so that nearly one half of it rested upon one bar, and the other half upon the other, the cap lying between the two. The needle was pressed close to the bars in this position, after which the bars were drawn away, both at the same time contrarywise, till they were clear of the needle; and this operation was repeated three or four times: after which that end of the needle which had rested upon the northern part of one of the bars, was found strongly to attract the north end of the needle in the compass-box; and the other end which had rested upon the southern part of the other bar, was found to attract in like manner the south end of the same needle in the box. The power of attraction also acquired by this needle appeared to be very considerable, it lifting easily with either of its ends, the two iron weights above mentioned, when cemented the one to the other with wax, and weighing together 1 ounce 10 pennyweights 5 grains.

The hard needle was then applied to the bars like the other and with the very same success, it lifted also, as the other had done, both the weights together.

The

The two needles were then themſelves applied to each other, firſt the northern half of the one, in a contrary direction, to the northern half of the other; and then the ſouthern half of the firſt, in a like contrary direction to the ſouthern half of the laſt; and from theſe ſeveral poſitions, they were ſeverally drawn till they were clear of each other, and this ſeveral times ſucceſſively: after which operation it was found, that the tempered needle had loſt ſo far its virtue, that its northern end had hardly any effect upon the needle in the box; that its ſouthern end even began to attract the contrary end of the needle from what it did before, and that it was no longer able to lift at either of its ends any ſenſible weight.

But as to the hard needle, that ſtill retained a conſiderable ſhare of its former virtue; its ends ſtill ſtrongly drawing the ſame ends of the needle in the compaſs-box as they drew before, and either of them lifting with eaſe the heavier of the two above-mentioned weights.

Mr. *Knight* then produced one of his common ſmall magnetic bars; the which being applied to the forementioned large bars, in the ſame manner as the needles had been applied to the ſame, but in a poſition contrary to that of its preſent polarity, it had its poles thereby counterchanged or inverted, and was found to lift at that which was now become its northern end, the weight of 6 ounces 8 pennyweight and 5 grains.

He laſtly produced one of his large artificial armed magnets, compoſed of ſeveral thin plates of ſteel cramped together, with which he acquainted us he had ſome time before lifted 36 pounds, and

and with which he did now actually lift before us 31 pounds 9 ounces and three fourths.

The temper'd needle ſpoken of above, and which had nearly loſt all its virtue, had the ſame again reſtored in great meaſure, upon being touched in the common way, on the armed poles of this artificial magnet; after which it diſcovered a ſtrong verticity, and was able to lift at one of its ends, the heavier of the 2 abovementioned weights, that is to ſay ſomewhat more than three quarters of an ounce.

The hard needle which ſtill retained, as has been obſerved, a conſiderable part of the virtue it had acquired by the touch of the large ſteel bars, was laſtly touched alſo in a contrary ſenſe, upon the armed poles of this artificial magnet; whereby it not only loſt the polarity yet remaining, but acquired a new one the other way, it would not however after this laſt touch lift more than nine pennyweight.

This is the true ſubſtance of the minutes I took, when theſe experiments were made, and which I preſume will now be verified by thoſe Mr. *Knight* is here prepared to ſhew.

AFTER the reading of this report, Mr. *Knight* did accordingly produce before the *Society* the two large bars and all the other particulars therein mentioned, with which he publicly repeated all the ſame experiments; which notwithſtanding the diſadvantagious circumſtances of the place, ſucceeded perfectly in every particular, and to the entire ſatiſfaction of all the company.

It

It was then further propoſed, that the temper'd needle, having its virtue again-deſtroyed, ſhould be touched upon the fine armed *Terella* belonging to the *Society*, which was the noble preſent of their late worthy member the Right Honourable *James* Earl of *Abercorn*, which is eſteemed one of the beſt in *England*, and is ſaid to have lifted in his Lordſhip's hands upwards of 40 pounds: the ſame was immediately brought, and the needle being touched therewith, was found to have acquired a ſtrong polarity, and to lift about the ſame weight, as when it was before touched upon Mr. *Knight's* large armed artificial magnet; that is to ſay about fifteen pennyweight.

II.

An Account of ſome new Experiments *lately made with* Artificial Magnets, *by the ſame.*

June 4, 1747.

Read July 2, 1747. THE *Apparatus* for touching of Needles, which I ſometime ſince had the Honour to ſhew before the *Royal Society*, was as perfect as I could have wiſh'd, as far as relates to the intended Uſe of it: But the manner in which the two Bars were diſpoſed in their Caſes made the Length of them ſomething incommodious, eſpecially in thoſe of the largeſt Size. This made me deſirous of trying if ſome Method could not be found out of placing the Bars parallel to each other without Danger of weakening their Force, by which means the Caſes would be reduced to half their Length. I remembered that ſome Years

ago,

ago I had tried ſome Experiments to this Purpoſe, by placing ſome Bars parallel and in Contact, but ſo that their Poles were turned different ways: in which Poſition I found the Virtue of ſome of them remain'd pretty entire, but that others were weakened thereby. I imagin'd the Reaſon of their loſing their Force was this; that the magnetic Virtue was by degrees habituated to paſs out of the Side of one Bar into that of the other in Contact with it, and thereby was hinder'd from arriving at the Ends in its full Vigour. The Reaſon why ſome ſuffer'd more than others was doubtleſs to be aſcrib'd to their Difference in Temper. I repeated the Experiment about two Months ago, with a little Alteration. I placed the Bars parallel with their Poles in an alternate Poſition, as before, but not in Contact, having kept them at the Diſtance of about a Quarter of an Inch. Then I apply'd to their Ends two Pieces of ſoft Iron. Each Piece was laid acroſs from the North End of one Bar to the South of the other, in the ſame manner as the Lifter is applied to the Feet of an armed Loadſtone. The Intent of this was to draw the magnetic Virtue thereby down to the Ends of the Bars, and to convey it through the Pieces of Iron from one to the other. In this Condition I let them lie for about a Month, and then tried if they would lift the ſame Weight as before, which I found they did, and I thought with more Vigour. After this I repeated the Experiment with other Bars of various Sizes, and with the ſame Succeſs: I have therefore now ventur'd to fit them up in Caſes in the manner juſt deſcribed.

The Succeſs of this Experiment had led me to another Improvement: I provided a Caſe of Braſs that would juſt contain two Bars, ſuch as are ſold for half a Guinea. At one End of the Caſe were fixed two Feet of ſoft Iron, like thoſe of an armed Load-ſtone, the upper Surface of which was within the Caſe in Contact with the Ends of the two Bars: which being parallel to each other, and their Poles in an alternate Poſition, the North-End of one Bar will be in Contact with one of the Feet, and the South End of the other Bar will be in like manner apply'd to the Surface of the other Foot. Upon fitting a Lifter to this new kind of Armour, I found I was able to ſupport a Weight of about 6 Pounds: The Bars are kept aſunder at the Diſtance of about a Quarter of an Inch, by a Slip of Wood, which ſlides in betwixt them.

An Inſtrument thus conſtructed ſeems capable of anſwering all the Purpoſes for which Loadſtones are uſed; for when the Bars are taken out of the Caſe, they are fit for touching Needles, or other magnetical Uſes, which may require ſingle Bars; when in the Caſe, the Whole together becomes an armed Magnet, able to lift a conſiderable Weight. And if we want to ſeparate iron Filings from thoſe of other Metals, the Feet and all the lower Part of the Caſe will take them up in great Plenty; and by drawing the Bars a little way out of the Caſe the Filings will fall off.

Some

IV.

Some further Experiments *relating to the general* Phænomena *of* Magnetism, *by the same.*

Read Dec. 17. 1747.

THE Cause of the surprizing *Phænomena* of the Loadstone has hitherto escaped our Knowledge, though diligently inquired after by Men of the Abilities. Such a Discovery is not to be made without long Experience, and a great Variety of Facts: And the Nature of the Subject is such, that the more Facts we are acquainted with, the more we find ourselves perplexed. The Conclusions we draw from some Experiments are seemingly contradicted by others; and yet these seeming Contradictions are oft-times very reconcileable upon further Experience. If what I am about to lay before the *Society* will in any-wise contribute to remove these Difficulties, I am in Hopes it will not be unacceptable, though I should not so properly explain the Nature of the Cause, as the Manner in which it acts. Many of these Experiments are not altogether new, but have not been so much attended to as they seem to deserve.

Proposition 1st.

The magnetic Matter of a Loadstone moves in a Stream from one Pole to the other internally, and is then carried back in curve Lines externally, till it arrives again at the Pole where it first entered, to be again admitted.

 Experiment

Experiment I.

If we lay a magnetical Body under a Piece of Paper or Glaſs that is ſtrewed over with ſteel Filings or magnetical Sand, and by ſtriking the Table put the Filings in Motion, they will readily diſpoſe themſelves in ſuch a manner as to repreſent, with great Exactneſs, the Courſe of the magnetic Matter. Steel rendered magnetical is beſt for this Purpoſe, becauſe it is of a more uniform Texture, than Loadſtones, and will on that account exhibit a more regular Appearance. By this Experiment the curve Lines in which the magnetical Matter returns back to the Pole where it firſt enter'd are accurately expreſſed by the Arrangement of the Filings. The largeſt Curves are ſuch as take their Riſe from one Polar Surface, and are extended to the other; being larger in proportion as they ariſe nearer the Axis or Centre of the polar Surface. Thoſe Curves which ariſe from the Sides of a magnetical Body are always interior to thoſe which ariſe from the polar Surface; and are leſs and leſs in proportion to their Diſtance from the Ends. If any one ſhould doubt, whether the magnetical Matter, which thus diſpoſes the Filings, is really moving back in a Direction contrary to that with which it paſſes through the magnetical Body; let him try it in different Parts with a ſmall Compaſs Needle, and the Fact will appear beyond Diſpute.

Exp. II.

The larger the Diſtance is from Pole to Pole in different Magnets the larger will theſe Curves be.

This

This appears from examining Magnets of different Lengths. And this is the Reaſon why in the ſame Magnet the Curves are leſs in proportion to their greater Diſtance from the Ends of the Bars. For the Poles from whence theſe Curves ariſe are proportionably nearer each other.

Exp. III.

If the South Pole of one Magnet be oppoſed to the North of another, moſt of the magnetic Matter is carried directly out of one into the other: and does not return back in curve Lines till after having paſſed through both Magnets. It appears from the Arrangement of the Filings that the magnetic Matter proceeding from the polar Surface does not now diverge from the Axis as before, but runs more in ſtreight Lines till it arrives at the polar Surface of the other Magnet. The Curves ariſing from the Sides, which before were bent towards the oppoſite End of the ſame Magnet, are many of them now bent the contrary Way towards the correſponding Sides of the other Magnet. Thoſe which are not bent the contrary Way are ſuch as are too remote from the oppoſed Pole of the other Magnet to be influenced thereby; and therefore continue their natural Courſe.

Exp. IV.

Whilſt the Bars are in the Poſition of the laſt Experiment; if a ſmall Loadſtone be placed in the Stream running from one to the other in any Poſition whatſoever, the Stream will paſs through the Stone:

Stone: which, being again removed, will be found to have a Polarity exactly in the Direction of that Stream.

Exp. V.

If the North or South Poles of two Magnets be opposed to each other, the Filings will exhibit the Appearance of two Streams meeting; and the Curves of each will all be turned towards the opposite Pole of the same Magnet. The Appearance is altogether the same, whether the two North or two South Poles be opposed to each other. So that it is not to be determined from any of these Experiments at which of the Poles the magnetic Stream enters! As we have some Reason to think it enters at the North Pole, we may suppose that the Case, without Danger of Errour; provided we build nothing upon the Supposition, but what would hold good (*mutatis mutandis*) if the contrary should be true. This being supposed, when the South Poles are opposite, the two Streams coming out at them are directly contrary, whereby the magnetic Matter is accumulated, and therefore diverges so much the faster to return back to the North Poles. When the North Poles are opposed to each other, the Streams of magnetic Matter returning from the South Poles are directly contrary; and by crouding at once towards each polar Surface are accumulated betwixt them, and converge towards them so much the faster.

These five Experiments seem sufficient to establish the Truth of the Proposition; and many more might be produced to the same Purpose.

Prop. 2.

The immediate Cause, why two or more magnetical Bodies attract each other, is the Flux of one and the same Stream of magnetical Matter through them.

Exp. VI.

It appeared in the third Experiment, that when the South Pole of one Magnet was opposed to the North of another, a Stream of magnetic Matter was carried from one to the other, and did not return back to the Pole where it first entered, till after having passed through both Bars: and it is needless to observe that two Bars in this Position are in a State of Attraction. The fifth Experiment shewed, that when the two South or North Poles were opposed, there was no Stream common to both. Now it is well known, that magnetical Bodies in this Situation are so far from attracting, that they strongly repel each other. If the third Experiment be repeated, with the Magnets placed at different Distances from each other, we shall find that more of the magnetical Matter will pass from one polar Surface to the other, in proportion as the Distance betwixt is less. The Attraction is therefore greater as the Distances diminish. And at Distances where none of the magnetic Stream passes from one Magnet to the other, there is no Sign of Attraction. So that this Cause is not only coexistent with the Effect, but also proportionable thereto.

Exp.

Exp. VII.

If a Piece of ſoft Iron which has no fixed Magnetiſm is any where placed in the magnetical Stream, it will be in a State of Attraction whilſt it remains in that Stream, and no longer.

Exp. VIII.

A Ball of ſoft Iron in Contact with the Pole of a Magnet will attract a ſecond Ball, and that a third, and ſo on, till the Stream becomes too weak to produce an Attraction ſufficient to ſupport a greater Weight.

Exp. IX.

Having hung a Number of Balls to each other, by applying the firſt to the North Pole of a Magnet, upon preſenting the South of another Magnet to one of the middle Balls; all thoſe below it will thereby be deprived of the magnetic Stream, and inſtantly loſing their Power of Attraction fall aſunder; the Ball, to which the Magnet was applied, will be attracted by it, and all the others will ſtill remain ſuſpended. But if the North End of a Magnet be preſented, then the Ball to which it is apply'd will alſo drop.

Exp. X.

In a Magnet unarmed the magnetic Stream is carried back on all Sides in curve Lines to the contrary Pole,

Pole, as was ſeen in *Experiment* I. but when Armour is applyed to each Pole, the magnetic Matter is thereby conducted to the Feet of the Armour; and a Lifter being thus apply'd to the Feet, the whole Stream coming out at one Pole is carried back through it to the other; by which means the Lifter is made to adhere to the Feet of the Armour with very great Force. When the Lifter is thus in Contact, the Magnet ſeems externally to have loſt the greateſt Part of its Force; though in Reality it never acted with more. If inſtead of the Lifter we ſuſpend a Number of Iron Balls in Contact, they will adhere together, and hang like a Bracelet betwixt the two Feet; the returning Stream paſſing now through them, as before through the Lifter. Preſent the Pole of a Magnet, and they inſtantly fall aſunder.

Prop. III.

The immediate Cauſe of magnetic Repulſion is the Conflux and Accumulation of the magnetic Matter.

It appeared in the fifth *Experiment*, that the ſame Poles of two different Magnets being oppoſed to each other, there was a Conflux and Accumulation of the magnetic Matter; and we find by Experience that all magnetical Bodies in a like Situation are in a State of Repulſion.

Exp. XI.

Two ſmall Bars, the one hard, the other of a Spring Temper, being both magnetical Matter, were

opposed to each other, South to South; the Filings produced the same Appearance of Repulsion, as described in the fifth *Experiment*; then the Bars being brought so near as to touch each other at the same Poles, the Repulsion was instantly changed into Attraction.

III. *A Discourse concerning the Usefulness of* Thermometers *in Chemical Experiments; and concerning the Principles on which the* Thermometers *now in Use have been constructed; together with the Description and Uses of a* Metalline Thermometer, *newly invented by* Cromwell Mortimer *M. D.* Sec. R. S. *&c.*

Read May 8. 1735. *here printed with some Alterations.*

CHEMISTRY being the most extensive Branch of Experimental Philosophy, hath furnish'd Mankind with the greatest Number of curious and useful Discoveries; for not only the Art of separating Metals from their Ores, of which Metals are form'd such Variety of useful Instruments, but likewise Cookery, which is so much concern'd about the Food of Mankind during Health, and also Pharmacy, which furnishes Medicines for the restoring Health when lost, the Art of Dyeing, and many other useful Manufactures, all owe their Improvements to this Science; many of which have been light on unexpectedly by the Operator, while he

he had ſomething elſe in View: but in many Caſes the Chemiſts complain, that, having once accidentally light on a curious Experiment, upon endeavouring to repeat it, they have never been able to make their Proceſs ſucceed exactly, as it did the firſt time, notwithſtanding that they made uſe of the ſame Materials, in the ſame Quantity, and conducted the Proceſs thro' exactly the ſame Operations. Where then muſt the Cauſe of the Miſcarriage lie? Surely in the Degree of Heat made uſe of in the two Experiments: For, in many common Operations, how uſual is it for a Preparation to be ſpoiled either by too little, or, moſt commonly, by too much Fire, too long or too ſhort a time applied! In order therefore to prevent theſe many Miſcarriages, I would adviſe the Chemiſt, in his Operations, to obſerve his Clock with as much Exactneſs as the Aſtronomer doth in his Obſervations; and in order to know to a Certainty the very Degrees of Heat he ever made uſe of in any Proceſs, that ſo he may be able to repeat and continue the ſame again in any Repetitions of the ſame Experiment, let him have his Laboratory furniſh'd with various Sorts of Thermometers, proportion'd to the Degree of Heat he intends to make uſe of. He will find theſe Inſtruments as uſeful to him in his Proceſſes, as they have proved to the curious Gardener in his Stoves, who by them is taught to keep his Plants in the ſame Degrees of Heat, as are natural to them in their reſpective Climates; which hath been ſet forth in Tables, after a very ingenious manner, by Mr.

 Sheldrake

* *Sheldrake* of *Norwich*. And besides the enabling him to perform his Operations with more Exactness, these Instruments would save him a great deal of Fuel; for as Liquors, while boiling, are not capable of receiving a greater Degree of Heat, all Fuel which is used more than to keep them in that State is useless; and the like happens in many other Cases.

These Instruments would also be of great Service to Maltsters, Brewers, Distillers, and Vinegar-makers; for, by Thermometers placed in different Parts of the Heap of wetted Malt, the proper Heat for its sprouting might be determined, and then regulated: The same for the Heat of the Kiln when the Malt is spread on it. By Thermometers the Brewer may ascertain the Heat of the Water when he pours it upon the Malt, the Heat of the Wort when he sets it to work, and the Heat while working: And in the like manner the Distiller and Vinegar-maker, in a Word, every Artificer, who employs Heat in his Business, may by these Instruments be certain of every Degree necessary in each Part of his Work.

Many Experiments shew, that all known Bodies, whether fluid or solid, increase their Bulk or rarefy by an Addition of Heat; and, on the contrary, contract or become more dense by the Diminution of Heat, which is the Presence of Cold: And these Alterations are always more or less sensible in proportion to the natural Rarity or Density of the Bodies. The

* Now Truss-maker over against the End of *Suffolk-street* near *Charing-Cross*, *London*, 1748.

The *Air* we live in, as it is the moſt rare and light Fluid, ſo are its Alterations the moſt ſenſible; and indeed I know of no Experiments which determine how far it is capable of being expanded by Heat, or condenſed by Cold; only we find that it will make its Way thro' any Fluid in which it lay dormant, when its elaſtic Property is rouzed by the Approach of ſuch an Heat as will make the Fluid boil. On the other hand, when compreſs'd by a Fluid ſo contracted by Cold, as to freeze, or become ſolid, its Elaſticity will only bear a certain Degree of Compreſſion, till the Force wherewith it endeavours to reſtore itſelf, exceeds the Force by which the Parts of the Solid, that confines it, adhere to each other; and ſo burſts its Priſon; as we often ſee during hard Froſts in Ice, and likewiſe Glaſs, and other hard Bodies, whoſe Parts cannot ſtretch.

Next to Air is *Alcohol*, or the higheſt rectified Spirit of Wine: This, Water, and all other Liquids, are capable of receiving no greater Degree of Heat than what makes them boil, as was firſt demonſtrated by Monſieur *Amontons*, a Member of the *Royal Academy des Sciences* at *Paris*; but that ingenious Inventor of the Quickſilver Thermometer Mr. *Fahrenheit* hath diſcover'd, that when the Barometer marks a greater Preſſure of the Atmoſphere, the ſame Liquor will receive 8 or 9 Degrees more of Heat than when the Barometer is at the loweſt. From hence the great Profeſſor *Boerhaave* gives the Hint, that, from nice Experiments being made of the different Degrees of Heat mark'd by a Thermometer in boiling Water compared with the different Heights of the Barometer, and Tables formed upon

upon them, a Thermometer applied to boiling Water might, at Sea, where the Motion of the Ship hinders Obſervations with the Barometer, ſerve to determine the Difference of the Gravity of the Atmoſphere. See his *Chemiſtry*, Tom. I. p. 171.

Theſe, and all other Liquids, by a certain determinate Degree of Cold peculiar to each ſort, loſe their Fluidity, and freeze, or become ſolid, but not in the ſame Order as by Heat they boil; for by Cold Oil or Water is ſooner frozen than Spirit of Wine, tho' Spirit of Wine will boil ſooner than Oil or Water. All ſolid Bodies likewiſe, as Minerals, Metals, and even Stones, will become fluid, or melt, at a certain Degree of Heat peculiar to each Species; and, when thoroughly melted, it is probable they are capable of receiving no higher Degree of Heat; and, on the Abſence of that Heat to a certain Degree, they all return to their natural ſolid State. Hence we may reaſonably conclude, that Solidity is the natural State of all Bodies; and that ſome are only accidentally fluid, becauſe their Conſtitution is ſuch as to melt by thoſe Degrees of Heat which our Atmoſphere is moſt commonly ſubject to. All ſolid Bodies are obſerved to contract themſelves into ſmaller Dimenſions by Cold, and gradually to expand themſelves at the Approach of Heat, till at laſt, being by Heat forc'd to the greateſt Degree of Expanſion, the Particles of which they are compoſed loſing their Coheſion, they become fluid; but no Experiments have yet been made, which determine whether Solids, expoſed to Cold beyond certain Degrees, will ceaſe to contract any more.

The learned Dr. *Muſchenbroek*, Prof. of Aſtronomy at *Utrecht*, and *F. R. S.* hath lately invented a very ingenious

ingenious Inſtrument, which he calls a *Pyrometer* and which Dr. *Deſaguliers* hath made ſome Improvements to *; a full Deſcription of which he hath given in his *Courſe of Experimental Philoſophy*, *Vol.* I. *p.* 421. *&c.* By this Inſtrument the Elongation of Rods of ſeveral Sorts of Metals by the Approach of a certain Number of Flames of a Spirit-Lamp, and likewiſe their as ſudden Contraction, on the extinguiſhing one or more of thoſe Flames, is render'd ſenſible to the Eye: Which ſufficiently evidences the Matter of Fact, and puts it beyond all Doubt.

From the above-mention'd Property of Bodies contracting and expanding in Cold and Heat, have all Thermometers been conſtructed, that have ever been made uſe of in order to obſerve and compare the different Degrees of Heat, either in our Atmoſphere, or in other Bodies. The moſt ſimple and moſt ſenſible of any is that aëreal Thermometer deſcribed by the great Mr. *Boyle*, in his New Experiments and Obſervations touching Cold, *Lond.* 1683. *4to. p.* 39. It conſiſts of a glaſs Bubble, with a very ſlender Stem not bigger than a Raven's Quill. The Bubble is left full of Air, and a few Drops of Water being convey'd into the Stem in an erect Poſition, will there remain ſuſpended to a certain Height; but, by the leaſt Addition of Heat, the Air in the Bubble expanding will puſh the Water up higher

* This Inſtrument hath ſince been greatly improved by that ingenious Watch-maker Mr. *John Ellicot*, *F. R. S.* See *Phil. Tranſ.* N°, 443.

higher; or, by the Approach of Cold, the Air contracting, the Water will fall lower in the Stem. This Instrument may be of Use in small Degrees of Heat, and in Cold, till the Water begins to freeze, when it becomes useless.

The next in Order of Sensibility is that first invented by *Cornelius Drebbelius* of *Alcmar*, and improved by *Boerhaave*. (See his *Chemistry*, Tom. I. p. 152, & 153.) It consists of an hollow glass *Lens* joined to a Stem of a larger Size than in the preceding, and a Bason into which the End of the Stem is inverted. The Air in the *Lens* must be so much rarefied, that the Stem being inverted into a tinged Liquor in the Bason, the Liquor will rise up some way in the Stem; then, by the Application of Heat to the *Lens*, the Liquor in the Stem will be push'd down, and by Cold the Liquor will rise up. This Instrument will give Notice of the smallest Changes in the Air; but it cannot be immersed into any Liquid for chemical Experiments, unless the Stem were made much longer, and bent down in Form of a Syphon: But even then it would be very unhandy, and, like the preceding, it would never serve for any Degree below what would freeze the Liquor made use of, nor for any above what would force out the confin'd Air through the Liquor in the Bason. Besides, both these Instruments, being subject to the Pressure of the Atmosphere, are not proper, without comparing the Barometer at the same time, to determine the Degrees of Heat at a great Distance of Time between each Experiment.

The

The moſt uſual Sort of Thermometers is that deſcribed in the Account of the Experiments by the *Academy* of *Cemento*; which being the common ones, made of Spirit of Wine ting'd, it is needleſs to deſcribe. The Bounds of the Degrees of Heat which theſe will meaſure, and which is commonly called the Range of the Inſtrument, are from the Degree which freezes Spirit of Wine, up to that which makes it boil. The Spirit-Thermometers, commonly made here in *London*, are ſo graduated, that when the Spirit is rarefied to the Degree that the moſt ſultry Sunſhine commonly known in our Climate of 51°½ N. Lat. can raiſe it, there is placed the Mark 0. or Degree of no Cold. Some few are mark'd 10 or 20 above this, if they are deſign'd to be uſed in hotter Climates; but all are graduated downwards from this: So that the 45°. is the Point of temperate, and 65°. is the Point of freezing, and 100°. is plac'd juſt above the Ball. But the moſt accurate Spirit-Thermometers are thoſe lately made by the ingenious Mr. *Reaumur*, Member of the *Royal Academy* of *Sciences* at *Paris*; he hath taken a great deal of Pains, and uſed great Exactneſs, in fixing the certain Points of freezing of Water, of temperate Air, and boiling Water. He determines the freezing Point, by leaving his Thermometer a conſiderable time in Water, into which is put a good deal of Ice, at a time when the Water would not freeze of itſelf; and this he marks 0. or the Degree of no Heat; and his Scale is mark'd with Numbers running downwards from 0. meaſuring the Degrees of Cold, and upwards meaſuring the Degrees of Heat: At 10½ upwards he marks

the Point of Temperate, which he determines by placing his Inſtrument in a ſubterranean Cavern, which is neither affected by Froſt nor Sunſhine, but is obſerved to keep an equable Temperature all the Year round; ſuch as deep Cellars and Wine Vaults commonly do. In boiling Water he finds that his Thermometer riſes to his 80th Diviſion,* or 80 Degrees, which are formed by dividing the Spirit when condenſed to the freezing Point, into 1000 equal Parts; ſo that, with the Heat which makes Water boil, the Spirit is expanded only $\frac{80}{1000}$, more than with the Cold which freezes Water.

Theſe Spirit-Thermometers are of Uſe in Experiments where ſomewhat greater Cold than the freezing of Water is required; but they can never be of Uſe in any Degrees of Heat beyond the boiling of the Spirit itſelf; becauſe it then becomes volatile, or riſes up in Steam, and not only expands no more, but likewiſe the Quantity is diminiſhed by the Particles which fly up from the Surface of the Liquor, and are ſuſpended in the Top of the Tube.

Many have filled their Thermometers with various Sorts of Oils †: Theſe indeed will meaſure many Degrees above the boiling of Water, till they boil themſelves; and then they have the ſame Defect as the Spirit

* But, with Submiſſion to ſo great a Man, I cannot apprehend that his Thermometers, when the Spirits are raiſed up to 80 do mark any greater Degree of Heat than their own ſpecific boiling Heat, which, if they are *Alcohol*, or the moſt rectified Spirits, anſwer to 174. of *Fahrenheit*'s Scale; if of the Strength of common Brandy to 190.

† See Dr. *Martin*'s Eſſays *Med. & Philoſ.* p. 225.

Spirit ones juſt mention'd, which is the Liquor loſing of its Bulk by Evaporation; and they congeal much ſooner than Water, and ſo are uſeleſs in meaſuring any Degrees of Cold.

The moſt uſeful Inſtruments, as they comprehend the largeſt Range, are the Mercurial Thermometers, which were brought into Uſe by that ingenious Artificer *Fahrenheit*, *F. R. S.* (See *Phil. Tranſ.* N°. 381.): But, to do Juſtice to a moſt worthy Member of the *Royal Society*, namely, Dr. *Halley*, he firſt gave the Hint, and even propoſed the making Thermometers of Quickſilver long before *Fahrenheit*'s Time (See *Phil. Tranſ.* N°. 197. p. 652.). However, *Fahrenheit* deſerves Thanks from the World for having brought theſe Inſtruments into Uſe, becauſe they will meaſure the greateſt Degrees of Cold yet known; for no Cold hitherto obſerved hath been able to freeze or render *Mercury* ſolid: And in meaſuring Heat, they go far beyond boiling Water, even beyond the melting of Tin or Lead. *Fahrenheit* begins his Scale from 0. the Point to which the *Mercury* hath been obſerved to fall by the greateſt Cold in *Iſland*; and computes, that the *Mercury* then * occupies 11124 Parts. This is his Point of no Heat. Then reckoning upwards from this, he finds that when the *Mercury* is rarefied only 32 Parts or Degrees more, common Water juſt begins to freeze: In a temperate Air it will riſe to about 60. The moſt ſultry Sunſhine ſeldom raiſes it to 90; the Heat of an animal Body to 96; the boiling of *Alcohol* to 174; the boiling of

* See *Boerh. Chem.* Tom. I. *p.* 174.

of Water to 212; and before the *Mercury* itſelf boils, it will riſe to 600.

I cannot here forbear giving an Abſtract of a very curious and ſurpriſing Experiment of *Fahrenheit*'s, concerning the artificial Production of Cold, as it is related by *Boerhaave* in his *Chemiſtry*, Tom. I. p. 164. *Fahrenheit* had a *Mercurial* Thermometer made with ſo long a Stem, that he could carry down the Scale 76 Parts or Degrees below 0. With this Inſtrument he found, that Cold might be produced by gradually pouring Spirit of Nitre upon powder'd Ice, till the *Mercury* would ſubſide to 40° below 0. that is 72° * lower than the Cold which freezes common Water. *Boerhaave*, in his *Chemiſtry*, Tom. I. p. 161. mentions a very pretty Way of determining the freezing Point: He adviſes to hang the Thermometer free in the open Air, not againſt any Wall or Building; and near it you muſt hang a Piece of very fine Linen or Muſlin juſt dipp'd in clean Water: When this begins to grow ſtiff, you will find the *Mercury* ſtand at about the 33d Degree; and it will alſo ſtand at the ſame Height when an hoar Froſt appears upon the Ground; which he looks upon as a certain Sign of the Beginning of freezing.

Having thus given an Account of the ſeveral Sorts of Thermometers hitherto uſed, and what Degrees of Heat they are proper to meaſure, we find none of them capable of meaſuring the greater Degrees of Heat, which are the moſt commonly made uſe of

* But what is this to the marvellous natural Cold of *Siberia*, 120°. below 0? See the Preface to *Gmelin*'s Flora Siberica. *Petrop.* 1747. 4to.

of by the Chemists in many of their Operations. Besides, all the above Instruments, being made of Glass, are easily broken by Accidents, and as liable to crack of themselves, by being taken out of a great Heat, and too suddenly exposed to Cold. I therefore consider'd whether the above-mention'd Property of Solids, and especially of Metals contracting with Cold, and expanding with Heat, might not be applied to the Construction of an Instrument capable of measuring all Degrees even of the greatest Cold, as well as the greatest Heat, to the melting Copper or Iron, which require more Heat than any other Metals to melt them. Altho' the Alterations in Metals are but small, in respect of those in Spirits, or even *Mercury*, yet it being found, that Iron, *e. g.* becomes $\frac{1}{60}$ longer * when red-hot, than when of its natural Temperature; and Dr. *Derham*, in his last Paper read before the *Royal Society* concerning the Vibration of *Pendulums*, says, that a Rod $39.\frac{126}{1000}$ Inches long, becomes $\frac{1}{10}$ Inch longer than its natural Dimensions in temperate Air, by being exposed to Heat equal to that of an human Body; $\frac{2}{100}$ Inch longer in hot Sunshine; that it was $\frac{2}{10}$ or $\frac{1}{5}$ Inch longer than its natural State, by being heated in a flaming Heat; that it became $\frac{7}{100}$ shorter than its natural Length by being quenched in cold Water; and still $\frac{3}{100}$ shorter, by being put into a Mixture of Salt and Snow. From which Experiments one may conclude, that from *Fahrenheit's* Cold of 40 below 0. to the greatest Heat Iron can bear without melting, a Rod of three Feet long will have about $\frac{1}{4}$ Inch Increase; which Increase of Length will be Range enough

* Vide *Sturm. Coll.*

enough to make all the intermediate Degrees obſervable upon an Inſtrument.

Suppoſe in *Fig*. 1. TAB. II. *AB* a Rod of Iron at its natural Length by the Heat of the Atmoſphere, placed upright upon one End; upon the Point of that reſts a Bar *CD* moveable on an Axis at *a*; and that, by making a Fire about the End *B* of the Rod, till it is juſt ready to melt, the Rod will increaſe in Length *Ab*, and conſequently puſh the Bar into the Situation *cd*. Now it is obvious to any one who underſtands ever ſo little of Mechanics, that tho' the Elongation of the Rod *Ab* be even ſcarce perceptible to the Eye, yet if upon the Bar *CD* the Diſtance *aA* from the Axis to the Place where the Rod *BA* puſhes againſt it be very ſmall, and the other Part of the Bar *aD* very long, the Arch *Dd* may be increaſed at Pleaſure, ſo as to bear to be divided into any Number of Diviſions that ſhall be found neceſſary: For the Arch *Dd* will always be to the Arch *Cc* in the ſame Proportion as the Diſtance *Da* is to *aC*; and likewiſe the Chords of theſe Arches *Dd* and *Ab* will be in the ſame Proportion; γ, δ, is the Situation of the Lever on the Level; and if it be found inconvenient to make the Arm *aD* ſo long, as to make very minute Alterations in the Length of the Rod *AB* eaſily obſervable, this Inconveniency may be readily removed by having a ſecond Bar *EF*, turning on the Axis *g*, whoſe Arm *gE* bearing up againſt the Extremity *D* of the firſt Bar or Lever, will riſe with it, or be preſs'd down by it; and the other Arm *gF* being lengthen'd at Pleaſure, the Arch *Ff* will be as large as you find convenient; or even a third and fourth Lever may be added.

When

When I first designed to have an Instrument constructed answering to the foregoing Principles of *Fig.* 1. I drew a Figure of it, wherein I proposed the Lever *AD* to have terminated in two Arches of Circles made out of one Piece of Brass; the smaller Arch formed on the Radius *aA* to be loaded with a Quantity of Metal sufficient to overcome all the Friction of the several Parts, so as to press down with a considerable Weight, and always to rest upon the Point *A* of the upright Rod *AB*; at *a* the Axis, on which they were to turn; and the larger Arch form'd on the Radius *aD*, was proposed to be a Sextant, the outward Edge of which was to be toothed, which Teeth were to play into the Teeth of a small brass Wheel carrying a steel *Index* like the Minute-hand of a Clock, which small Wheel with the Hand was to make one Revolution nearly by the utmost Rise and Fall of the Sextant at *D*; or, instead of Teeth, I proposed a Piece of a Watch-Chain to be fasten'd to the upper Limb of the Sextant, and so to be brought downwards, and passing nearly round the small Wheel in one Groove, to be fasten'd to it: In another Groove in this small Wheel was another Piece of Watch-Chain to be fasten'd, which, being passed contrarywise round the said Wheel, was to have a Weight hung to it that would be a Counterpoise to the Sextant; but, upon consulting my two ingenious Friends Mr. *Geo. Graham* and Mr. *John Ellicot* two worthy Members of this *Society*, they each of them persuaded me to lay aside that more complexed Construction, and to have the Instrument made in the plain and simple manner in which Mr. *Jackson*

Jackson executed it for me in the Year 1736. as is represented in *Fig.* 2 and 3. TAB. II.

The Description of the Instrument.

Fig. 2. *AB* a round Rod of Steel or Brass a Quarter of an Inch thick, and 3 Feet 1 Inch long: When the Rod is of Brass 3 Feet long, the Point *A* must be of Steel 1 Inch long, to prevent its wearing away, or losing its Point; which conical Point is made to screw on and off.

I had the first Rods made $1\frac{1}{2}$ Inch thick at *B*, and of the same Thickness 6 Inches up; but I found Inconveniences from that Form, and that a Rod all of a Size was better.

CD, *cd*, are two iron Supporters, joined by a flat cross Bar at Bottom *Dd* two Inches long, in the middle of which is a Point $\frac{3}{8}$ Inch high under *B*, which goes into an Hole at the Bottom of the Rod *B*, and serves to keep the Rod in its Place at Bottom, as the cross Bar ** having an Hole in it, thro' which the Rod passes, does in the middle or about $\frac{2}{3}$ up the Supporters, and the Point *A* goes into a small Hole in the under Side of the Lever; all which keep the upright Rod firm and steady in its Place. The iron Supporters are flat, or parallel to the Front of the Machine from *C* to *X* and *c* to *x*, where they are twisted half-round, so that the lower Parts *XD*, *xd* stand at right Angles with the upper Parts. This Contrivance gives the freer Access to the Rod for the Sand or Fluid into which the Machine is set to measure the Heat of it, the Supporters standing 2 Inches asunder at *Dd*; and that the Degrees of Heat

2 may

may be compared uniformly in different Experiments, the Bottom of the Rod must always be immersed to the same Height in the Matter to be examined; and therefore I make a Mark, a small Furrow † quite round the Rod, 1½ Inch from the Bottom *B*. For the deeper the Rod is immersed into any Matter, it will be lengthen'd the more by the same Degree of Heat.

EF, the Lever, which turns upon an Axis *G*. At *F* is fastened a String, which, passing twice round the small Pulley *H*, has a Weight *I* hanging to the other End of it, of about half a Pound, being enough to keep the String always stretch'd. At the other End *E* of the Lever is hung another Weight *L*, which must be heavy enough not only to counterbalance the longer Arm *GF*, but press down upon the Point *A* with a Weight sufficient to keep its steady.

MNO, is the back Part of the Plate, like the Dial-Plate of a Clock made of Brass. See the Front of it at *Fig.* 3.

The Pulley *H* turns upon an Axis *C* in *Fig.* 3. which goes thro' the Plate, and on the other Side or Front of the Dial-Plate carries a Hand or Index *AB* in *Fig.* 3.

N. B. *G* being the *Fulcrum* of the Lever, the Distance *GA* being very small, and the Distance *GF* being very great, the smallest Motion at *A* will produce a very great one at *F*, and therefore the Index will turn very sensibly upon the Plate.

The Proportions of the Rod and Lever are discretionary; my Rods both of Steel and Brass are 3 Feet long in one solid Piece, but they have each a Point or Cone of Steel 1 Inch high, that screws

upon the Top at *A*. The Lever has 4 Inches from *E* to *A*, $1\frac{1}{7}$ Inch from *A* to *G*, and 12 Inches from *G* to *F*; the Diſtance of *G* above *c* is $1\frac{1}{2}$ Inch, the braſs Pulley *H* is $\frac{1}{2}$ Inch Diameter; all the other Parts of the Machine are of Oak. The main Support or Pillar *PQ* is 1 Inch ſquare, $2\frac{1}{2}$ Feet high, and at Bottom is let thro' a Groove at *Q* made in a great heavy Block or Pedeſtal of Wood *RS*. In this Groove the Pillar may be raiſed higher or lower, in order to adjuſt the Height of the Pillar to the Situation, which the Bottom of the Rod *AB* may require in different Experiments; and it is to be fixed in that Place by a Screw at *T*, which goes thro' the Front of the Block, and preſſes againſt the Bottom of the Pillar.

Fig. 3. repreſents the *Dial-Plate*, or Front of the Plate mark'd *MNO*: In *Fig.* 2. it is a Plate of Braſs, with ſtrong Paper glued upon it, and may be of what Size you pleaſe; mine is 11 Inches over.

AB is the Hand or *Index*, which ſlips on very ſtiff upon the Axis *C*, that carries the Pulley *H* in *Fig.* 2. The outer Circle is to be left wide enough to contain the chemical Characters or Marks which are to be made upon it, the Arch *DE* contains the Diviſions of *Fahrenheit*'s Mercurial Thermometer; the Arc *FG* thoſe of *Reaumur*, or the Spirit of Wine Thermometer.

In order to adjuſt this Inſtrument for Uſe I place the Bottom of the Rod *B* in *Fig.* 2. immerſed up to the Mark ✝ in cold River or Rain Water, in a Veſſel proper to be ſet over the Fire; and when it

has

has boiled for a Quarter of an Hour, I turn the Index *AB* in *Fig.* 3. till it ſtands in the horizontal Poſition, as at *B*, being the Point of boiling-hot Water, and which anſwers to Diviſion 212 on *Fahrenheit's* Arch. I then take it out of the Water, and dry it, by holding it a little over the Fire: And now great Care muſt be taken, that nothing alters the Situation of the Index upon the Axis; even a Nut to ſcrew on upon the Axis at *C* may be the beſt to keep it fixed. If the Inſtrument be left to cool in the Air, the Index will fall below *B*, ſhewing the Degrees of Cold, or leſs Heat than boiling Water; and if put into melting Tin, Lead, *&c.* it will ſhew the Degrees of Heat above boiling Water. A braſs Rod will ſerve for an Inſtrument to meaſure the greateſt Degrees of Cold, and all the Degrees of Heat, to the melting of Silver or Gold; but if you have a Mind to make one to meaſure greater Degrees of Heat, the Rod muſt be of Steel, or the fineſt Iron. A Rod of Braſs, according to Dr. *Muſchenbroek's* Experiments, *l. c.* was found to lengthen 377, when one of Iron lengthen'd only 230 Parts. An iron Rod, being regulated by boiling Water, as above directed, will meaſure not only the Heat of melted Tin and Lead, but of Silver, Gold, and Copper, and will even ſhew the Degree when Iron itſelf begins to melt, which will be the greateſt Degree of Elongation of the Rod juſt before the Bottom of it runs; and I imagine, that an Inſtrument may be conſtructed with Supporters, and a Rod made of Tobacco-pipe Clay, which, being regulated by boiling Mercury (for it muſt never touch Water), may

 be

be adapted to meaſure ſtill greater Degrees of Heat, till the Materials themſelves melt into Glaſs.

I ſhould adviſe, that not only the Scale of this ſort of Thermometer, but likewiſe of all others, be determined by Experiments, without regarding any Equality as to Meaſure between the Diviſions, and that in every Individual that ſhall be made; for a Difference in the Length and Thickneſs of the Rods in this ſort will make a Difference in the Scale, as much or more than the Inequality in the Cavity of the Stem, or glaſs Tube of other Thermometers, which can never be juſt, if applied to a Scale whoſe Diviſions are made equal; unleſs the Cavity of the Stem be perfectly equal, which it is impoſſible for any Workman to undertake to do, and which is very ſeldom, if ever, hit on by chance. Therefore, in theſe Inſtruments, let the Point *B* in *Fig.* 3. or the horizontal Poſition of the Index, be the Situation of the Index when the Rod has ſtood a Quarter of an Hour in boiling Water; there mark ▽ *boiling* on the outer Circle; on *Fahrenheit*'s Arch mark 212. then ſet your Machine up to the Mark + into melting Tin, which is the Metal that melts eaſieſt. When the Rod is arrived to its greateſt Elongation in that Metal, inſcribe the Character ♃ on the outer Circle; do the like with Lead, and ſet the Character ♄ at it. At the boiling of Mercury put the Mark ☿, and on *Fahrenheit*'s Arch mark 600. the utmoſt Extent his Mercurial Inſtruments can meaſure: Then proceed to the melting of Silver, and ſet the Mark ☾; at the melting of Gold place the Mark ☉; at the melting of Copper place the Mark ♀; at the melting of Iron place the Mark ♂, the moſt difficult to melt of all Metals.

As

As the Divisions pointed out by the Index will be different with Rods of different Metals or Substances, you may make different Circles upon the Plate for the Range of the different Rods, and mark them; the Iron Rod, the Brass Rod, the Clay Rod; and set the several Marks above specified upon each Circle apart; or you may, to avoid Confusion, have a different Instrument for each kind of Rod.

Being obliged to take down my *Athanor* and Wind-Furnace, upon removing twice to different Houses, and not having rebuilt them where I now live, I have not had Opportunities yet of fixing the Scale of my own Instrument, which was one Reason why I did not publish an Account of my Invention sooner; for I hope hereafter to be able to compare the Degrees of Heat necessary for the melting of each Metal, and to determine the Question whether Metals in the highest Degree of Fusion are susceptible of greater Degrees of Heat by increasing the Fire, as Water thoroughly boiling can never be made hotter; nor did I intend to have publish'd any Description of this Instrument till I had completed Tables of the Degrees of Cold and Heat, from *Fahrenheit*'s Experiment of Cold produced by Art 40 Degrees below 0 to the Heat of melting Iron.

According to *Fahrenheit*'s Scale, the Heat of the strongest Sunshine is at about 80. Spirit of Wine boils at 176. Water at 212. the *Lixivium* of Salt of Tartar at 240. Spirit of Nitre at 242. Oil of Vitriol at 546. Quicksilver at 600 *.

As

* *See* Augustin. Grischow *Thermometria comparata accuratius, & Harmonica.* Berolini 1740. *4to.* p. 10.

As all chemical Digeſtions, where an equable Heat is to be continued for ſome time together, will come in between hot Sunſhine and the boiling of Quickſilver, a Thermoſcope of that Range will be ſufficient for common Uſes; and therefore one fitted with a braſs Rod will anſwer theſe Purpoſes.

In large Furnaces for running down Ores, or melting great Quantities of Metal together, it is not poſſible to place ſuch an Inſtrument; but then in Lead and Tin there may be ſmall Outlets contrived, into which ſome of the melted Metal may be permitted to flow, and remain in Contact with the ſame Body of Metal within, where the Inſtrument may be placed; and for placing a Thermoſcope in Iron, Copper, or Glaſs Furnaces, there may be a Place contrived, which ſhall not open into the Furnace, but have the Thickneſs of a Stone or Brick left between, upon which the Inſtrument may be plac'd; and tho' in ſuch a Situation it will not meaſure the actual Heat within the Furnace, it will always give the relative or comparative Heat in the like Circumſtances at different Times, and ſo ſhew us how to regulate the Heat within.

Altho' a Chemiſt ſhall have one of theſe Inſtruments to meaſure the Heat, he ſhall have uſed in any Experiment, and have noted down the ſeveral Degrees made uſe of, and the Time each laſted, he ſtill labours under another Difficulty, which is the not being able to command any required Heat, and that it ſhall laſt a certain required Time, unleſs it be below that of boiling Water, which may be procured and continued by various Contrivances of Lamps, either of Spirits, or of Oil; but how to continue a Fire for 12 or 24 Hours together, without Attendance, which ſhall continually keep Quickſilver boiling,

boiling, Lead in Fusion, or may be let down so low as not to exceed the Heat of Sunshine, and then be raised again, and that without letting out the Fire, or moving the Vessels, may seem almost impracticable; but by an Improvement of the Furnace the antient Chemists call'd their *Athanor*, I hope to succeed in it, which may be the Subject of another Paper.

The Rev. *Stephen Hales* D. D. that most worthy Member of the *Royal Society*, to whom the World is greatly indebted for many accurate Experiments, and useful Discoveries, upon hearing the Minutes of my Paper deliver'd in to the *Royal Society*, on *May* 8. 1735. read upon the *Thursday* following, desired me to lend him the Original for some Days, telling me he had some Thoughts of making a Thermoscope with a Rod of Lead. After a few Days he returned me my Paper, with the following obliging Letter, and kind Remarks.

SIR,

I HAVE read over your *Thermometrical Tract* with Satisfaction, and believe it will be of good Use. The Want of ascertaining the Degrees of Heat and Cold is a great and important *Desideratum* in Experimental Philosophy.

What I intended to do was only this, *viz.* to get a leaden Wire, of such a Size and Strength as to bear its own Weight, to have it as long as the longest Gun-barrel I could procure, and to have it sustain a Lever as you have done; then to pour boiling Water into the Barrel, for a long time, till the Lever rises

rises no more; the Water to have Vent at the Bottom, yet so as to have the Gun-barrel always full of Water; the Breech-Pin to be out, and the leaden Rod to rest on a Piece of Wood set upright, according to the Course of its Fibres, not sideways.

To give at the same time to a Mercurial Thermometer the Heat of boiling Water.

Then to take the freezing Point of the Leaden and Mercurial Thermometers; and afterwards to graduate all the intermediate Degrees, from the Mercurial Thermometer upon the Leaden Thermometer, as they occur.

Thus a Standard Thermometer may be made to graduate others by; but I will not now set about it, since you have undertaken the Subject.

His *Remarks* on the foregoing Paper.

Page. 673. *Thermometers* must be of excellent Use in Garden-Stoves; but foreign Plants must not be kept in an equal Degree of Heat in Stoves, to that of their native Country; *viz.* because they cannot bear as great a Heat in a confined close Air, as in an open free Air. I have been told of Coffee-Trees being killed here in *England* by this Mistake: Such Plants must doubtless be kept warm*, but not so warm as in their native Country.

P.

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* I should think it best to lessen the Heat in Stoves towards the Night, and so to keep the Plants exposed to less Degrees of Heat a-nights than a-days, nay to vary the Heat daily, or to endeavour by Art to procure different Degrees of Heat, agreeable to the natural Vicissitudes of the Climate the Plants come from, having Regard both to the Seasons of the Year, and the State of flowering or Fructification of the Plants; so that the best Way of ranging Plants in Green-houses or Stoves is according to the Climates they come from; for which Mr. *Sheldrake*'s Tables above-mentioned, *p.* 674. must be of excellent Use. *C. M.*

P. 676. [*All solid Bodies are observed to contract with Cold.*] I have found that Wood does not contract or dilate lengthways with Heat or Cold. I am told that Mr. *George Graham* [is about making] this Experiment, as I am also, in order to regulate *Pendulums.*

P. 682. I fear that *Boerhaave*'s wet Linen, which is so thin, may begin to freeze before all the *Mercury* or Spirit of Wine in the Ball of the Thermometer has the same Degree of Cold: Tho' hanging there long before and after freezing will bring it pretty near.

P. 683. [*A Rod of* Iron 3 *Feet long will have about* $\frac{1}{4}$ *Inch Increase*] or $\frac{1}{144}$th Part.

IV. *A Continuation of a Paper concerning* Electricity, *by* William Watson *F. R. S. printed in these* Transf. N. 477, Article I. *ending* p. 501.

Read Feb. 6. 1745-6.

AS Water is a Non-electric, and of consequence a Conductor of Electricity, I had Reason to believe that Ice was endowed with the same Properties. Upon making the Experiment, I found my Conjectures not without Foundation; for, upon electrifying a Piece of Ice, wherever the Ice was touched by a Non-electric, it flashed and snapped. A Piece of Ice also held in the Hand of an electrified Man, as in the before-mentioned Processes, fired warm Spirit, chemical vegetable Oils, Camphor, and Gunpowder prepared as before. But here great Care must be taken, that,

by the Warmth of the Hand, or of the Air in the Room, the Ice does not melt; if ſo, every Drop of Water therefrom conſiderably diminiſhes the received Electricity. The Experiment will ſucceed likewiſe, if, inſtead of the Ice, you electrify the Spirit, *&c.* and bring the Ice not electrified near them. I muſt obſerve, that Ice is not ſo ready a Conductor of Electricity as Water; ſo that I very frequently have been diſappointed in endeavouring with it to fire inflammable Subſtances, when it has been readily done by a Sword, or the Finger of a Man.

In the firſt Paper * I had the Honour to lay before you upon this Subject, I took notice of my having obſerved two different Appearances of the Fire from electrified Subſtances; *viz.* thoſe large bright Flaſhes, which may be procured from any Part of electrified Bodies, by bringing a Non-electric unexcited near them, and with which we have fired all the inflammable Subſtances mentioned in the Courſe of theſe Obſervations; and thoſe, like the firing of wet Gunpowder, which are only perceptible at the Points or Edges of excited Non-electrics. Theſe laſt alſo appear different in Colour and Form according to the Subſtances from which they proceed: For from poliſhed Bodies, as the Point of a Sword, a Silver Probe, the Points of Sciſſors, and the Edges of the Steel Bar made magnetical by the ingenious Dr. *Knight*, the electrical Fire appears like a Pencil of Rays, agreeing in Colour with the Fire from *Boyle*'s Phoſphorus; but from unliſhed

* Phil. Tranſ. *Vol.* 43. *Page* 483.

polished Bodies, as the End of a Poker, a rusty Nail, or such-like, the Rays are much more red. The Difference of Colour here, I am of Opinion, is owing rather to the different Reflexion of the electrical Fire from the Surface of the Body from which it is emitted, than to any Difference in the Fire itself. These Pencils of Rays issue successively as long as the Bodies, from which they proceed, are exciting; but they are longer and more brilliant, if you bring any Non-electric not excited near them, tho' it must not be close enough to make them snap. If you hold your Hand at about two or three Inches Distance from these Points, you not only feel successive Blasts of Wind from them, but hear also a crackling Noise. Where there are several Points, you observe at the same time several Pencils of Rays.

It appears from Experiments, that, besides the several Properties that Electricity is possess'd of peculiar to itself, it has some in common with Magnetism and Light.

Proposition I.

In common with Magnetism, Electricity counteracts, and, in light Substances, overcomes the Force of Gravity. Like that extraordinary Power likewise, it exerts its Force *in vacuo* as powerfully as in open Air; and this Force is extended to a considerable Distance through various Substances of different Textures and Densities.

Corollary.

Gravity is the general Endeavour and Tendency of Bodies towards the Center of the Earth; this is overcome by the Magnet with regard to Iron, and

 by

by Electricity with regard to light Subſtances both in its Attraction and Repulſion; but I have never been able to diſcern that vortical Motion, by which this Effect was ſaid to be brought about by the late Dr. *Deſaguliers* and others, having no other Conception of the Manner of its acting than as Rays from a Centre, which indeed is confirmed by ſeveral Experiments: One of which, very eaſy to be tried, is, that if a ſingle downy Seed of Cotton-graſs is dropped from a Man's Hand, and in its Fall comes within the Attraction of the rubbed Tube, the Down of this Seed, which before ſeemed to ſtick together, ſeparates, and forms Rays round the Center of the Seed. Or if you faſten many of theſe Seeds with Mucilage of Gum Arabic round a Bit of Stick, the Down of them, when electrified, which otherwiſe hangs from the Stick, is raiſed up, and forms a circular Appearance round the Stick. As theſe light Bodies are directed in their Motions only by the Force impreſſed upon them, and as their Appearance is conſtantly *radiatim*, ſuch Appearance by no means ſquares with our Idea of a Vortex. Some have imagined a Polarity alſo, when they have obſerved one End of an excited glaſs Tube repel light Subſtances, and the other attract them. But this is a Deception ariſing from the whole Length of the Tube not being excited, but only ſuch Part of it as has been rubbed; ſo that as much of the Tube as is held in the Hand remains in an unexcited State, and permits light Subſtances to lie ſtill thereon, though forcibly repell'd at the other End. This attractive Power of Electricity acts not only upon Non-electrics, as Leaf-Gold, Silver, Thread, and ſuch-like,

but

but alſo upon Originally-Electrics, as Silk, dry Feathers, little Pieces of Glaſs, and Reſin: it attracts all Bodies, that are not of the ſame Standard of Electricity (if I may be allowed the Expreſſion) as the excited Body from which it proceeds. I have found no Body, however denſe, whoſe Pores are not pervious to Electricity by a proper Management, not even Gold itſelf.

Propoſition II.

In common with Light, Electricity pervades Glaſs, but ſuffers no Refraction therefrom; I having, from the moſt exact Obſervations, found its Direction to be in right Lines, and that through Glaſſes of different Forms, included one within the other, and large Spaces left between each Glaſs.

Corollary.

This rectilineal Direction is obſervable only as far as the Electricity can penetrate through unexcited Originally-Electrics, and thoſe perfectly dry; nor is it at all material, whether theſe Subſtances are tranſparent, as Glaſs; ſemidiaphanous, as Porcelain, or thin Cakes of Wax; or quite opaque, as thick woollen Cloth, as well as woven Silk of various Colours; it is only neceſſary that they be Originally-Electrics. But the Caſe is widely different with regard to Non-electrics; wherein the Direction, given to the Electricity by the excited Originally-Electric, is alter'd as ſoon as it touches the Surface of a Non-electric, and is propagated with a Degree of Swiftneſs ſcarcely to be meaſured in all poſſible Directions to impregnate the whole non-electric Maſs in Contact with it, or nearly ſo, however different in itſelf, and which muſt of Neceſſity be terminated by an

Originally-

Originally-Electric, before the Electricity exerts the least Attraction; and then this Power is observed first at that Part of the Non-electric the most remote from the Originally Electric. Thus, for Example, by an excited Tube held over it, Leaf-Gold will be attracted thro' Glass, Cloth, &c. held horizontally in the Hand of a Man standing upon the Floor, and this Attraction is exerted to a considerable Distance. On the contrary, the rubbed Tube will not attract Leaf-Gold, or other light Bodies, however near, through Silver, Tin, the thinnest Board, Paper, or any other Non-Electric, held in the manner before-mention'd. But if you rub the Paper over with Wax melted, and by that means introduce the Originally-Electric therein, you observe the Electricity acts in right Lines, and attracts powerfully. And here I must beg Leave to remind you, not only of the former Corollary, but of some of the former Experiments also; by which it appears, that although, to make a Non-Electric exert any Power, we must excite the whole Mass thereof, yet we can excite what Part, and what only, of an Originally-Electric we please. Thus we observe, that Leaf-Gold, and the Seed of Cotton-Grass (which grows upon Bogs, and is a very proper Subject for these Inquiries) are attracted under a glass Jar made warm*, and turned Bottom upwards, upon which are placed Books, and several other

* I have constantly observed, that the electrical Attraction through Glass is much more powerful when the Glass is made warm, than when cold. This Effect may proceed from a twofold Cause: *First*, warm Glass does not condense the Water from the Air, which makes the Glass, as has been before before demonstrated (*p.* 111) a Conductor of Electricity: Secondly; As Heat enlarges the Dimensions of all known Bodies, and consequently causes their constituent Parts to recede from each other, the electrical Effluvia, passing in straight Lines, find probably a more ready Passage thro' their Pores.

other Non-Electrics; and that the Motions of the light Bodies underneath correſpond with the Motions of the glaſs Tube held over them, the Electricity ſeeming inſtantaneouſly to paſs thro' the Books and the Glaſs. But this does not happen, till the Electricity has fully impregnated the Non-electrics, which lie upon the Glaſs; which received Electricity is ſtopped by the Glaſs; and then theſe Non-Electrics dart their Power directly through the upper Part of the Glaſs, after the Manner of Originally-Electrics. But if the thinneſt Non-Electric, even the fineſt Paper, as I before mentioned, is held in the Hand of a Man at the ſmalleſt Diſtance over the the Leaf-Gold, and the Electricity is not ſtopped, not the leaſt Power will be exerted, and the Gold will lie ſtill. I muſt here remark likewiſe, that this Law of Electricity is ſo conſtant and regular, that I have not found one Deviation from it; ſo that even the Quickſilver, ſpread thin, as it uſually is at the Back of a Plate of a Looking-Glaſs, will prevent the paſſing through of the electrical Attraction, unleſs ſtopped by an Originally-Electric. This Penetration of the electrical Power through Originally-Electrics is much greater than has hitherto been imagined, and has cauſed the Want of Succeſs to great Numbers of Experiments. I have been at no ſmall Pains to determine, how far this Power can penetrate through a dry Originally-Electric, and have found, by repeated Trials, that either in a Cake of Wax alone, or of Wax and Reſin mixed, when the Electricity is very powerful, it has paſſed, I ſay, in ſtrait Lines through theſe Cakes of the Thickneſs of two Inches and $\frac{4}{10}$; but I never could make it act through one of two Inches $\frac{8}{10}$; for in this it was perfectly

ſtopped. So that the Cakes commonly made uſe of to ſtop the Electricity, by being too thin, ſuffer a conſiderable Quantity of the electrical Power to pervade them, and be loſt in the Floor. I make no Doubt, if the electrical Power could be more increaſed, it would penetrate much further through theſe Originally-electric Bodies.

Propoſition III.

Electricity, in common with Light likewiſe, when its Forces are collected, and a proper Direction given thereto upon a proper Object, produces Fire and Flame.

Corollary.

The Fire of Electricity (as I have before obſerv'd) is extremely delicate, and ſets on Fire, as far as I have yet experienced, only inflammable Vapours. Nor is this Flame at all heighten'd by being ſuperinduced upon an iron Rod, red-hot with coarſer culinary Fire, as in a preceding Experiment; nor diminiſh'd by being directed upon cold Water. However, I was deſirous of knowing if this Flame would be affected by a ſtill greater Degree of Cold; and in order to determine this, I made an artificial Cold, by which the Mercury, in a very nice Thermometer adjuſted to *Fahrenheit*'s Scale, was depreſſed in about 4 Minutes from 15 Degrees above the freezing Point to 30 Degrees below it; that is, the Mercury fell 45 Degrees. From this cold Mixture, when electrified, the Flaſhes were as powerful, and the Stroke as ſmart, as from the red-hot Iron. I could have made the Cold more intenſe, but the above was ſufficient for my Purpoſe. This Experi-

ment

ment ſeems to indicate, that the Fire of Electricity is affected neither by the Preſence or Abſence of other Fire. For, as red-hot Iron, by Sir *Iſaac Newton's* Scale of Heat, is fixed to 192 Degrees, and as the Ratio between Sir *Iſaac's* Degrees and *Fahrenheit's* is as 34 to 180, it neceſſarily follows, that the Difference of Heat between the hot Iron and the cold Mixture is 1040 Degrees; and nevertheleſs, this vaſt Difference makes no Alteration in the Appearance of the electrical Flame. We find likewiſe, that as the Fire, ariſing from the Refraction of the Rays of Light by a *Lens*, and brought to a *Focus*, is obſerved firſt at ſome ſmall Diſtance from their Surfaces, to ſet on fire combuſtible Subſtances; the ſame Effect, as I have before obſerv'd, is produced in like manner by electrical Flame.

I may perhaps be thought too minute in ſome of the before mention'd Particulars; but in Inquiries abſtruſe as theſe are, where we have ſo little *a priori* to direct us, the greateſt Attention muſt be had to every Circumſtance, if we are truly deſirous of inveſtigating the Laws of this ſurpriſing Power. For, as has been ſaid upon another Occaſion, by my ever honour'd Friend *Martin Folkes* Eſq; our moſt worthy Preſident, "That Electricity ſeems to furniſh an in-" exhauſtible Fund for Inquiry; and ſure *Phænomena* ſo various and ſo wonderful can ariſe only "from Cauſes very general and extenſive, and ſuch "as muſt have been deſigned by the Almighty Au-" THOR of NATURE for the Production of very "great Effects, and ſuch as are of great Moment to "the Syſtem of the Univerſe."

If theſe Obſervations receive the Countenance of this Learned *Society*, I ſhall think myſelf ſufficiently recompenſed; and am,

Gentlemen, with the higheſt Eſteem,

Your moſt obedient humble Servant,

W. Watſon.

V. *A* Sequel *to the Experiments and Obſervations tending to illuſtrate the Nature and Properties of* Electricity; *in a Letter to the* Royal Society *from the ſame.*

Gentlemen,

Read Oct. 30. 1746.

1. THE favourable Reception wherewith you honour'd ſome Papers I laid before you ſome time ſince, relating to Electricity, emboldens me to trouble you again upon the ſame Subject: And I am the more encouraged ſo to do, as the Progreſs of our Diſcoveries therein, both here and abroad, has been ſo rapid; that what, little more than a Year ago, we conceived to be the *ne plus ultra* of our Inquiries, is now regarded as mere Rudiments.

2. It were treſpaſſing too much upon you, to recount the great Number of Experiments I have made; for which Reaſon I ſhall only take notice of ſuch as are either in themſelves ſtriking, or tend to illuſtrate ſome Propoſition.

3.

3. At the Beginning of laſt Summer I cauſed a Machine to be made for electrical Purpoſes; the Wheel whereof was four Feet in Diameter. In the Periphery of this Wheel were cut four Grooves, correſponding with four Globes of ten Inches Diameter, which were diſpoſed vertically at about three Inches Diſtance from each other. One, two, or the whole Number of theſe Globes might be uſed at Pleaſure. They were mounted upon Spindles of two Inches Diameter, and their mean Motion round their Axis was about éleven hundred times in a Minute. As it is next to impoſſible to have theſe Globes blown and mounted perfectly true, I order'd the Leather Cuſhions, with which they were rubb'd, to be ſtuffed with an elaſtic Subſtance (curled Hair) that the Globes in their Rotations might be as equally rubb'd as poſſible. You might likewiſe cauſe the Globes to be rubb'd by the Hands of your Aſſiſtants; but under a certain Treatment (of which hereafter) the Cuſhions excite equally ſtrong. The Leather Cuſhions were now-and-then rubb'd over with Whiting. As a minute Detail of the Parts of this Machine would take up too much of your Time, I have herewith laid before you a Draught thereof.

4. I lined one of theſe Globes to a conſiderable Thickneſs, with a Mixture of Wax and Reſin, in order to obſerve whether or no the Electricity would be the ſooner or more ſtrongly excited; but I found no Difference in the Power of this Globe from the others, which were without this Treatment.

5. The Power of Electricity is increaſed by the Number and Size of the Globes to a certain Degree;

but by no means in proportion to their Number and Size: Therefore, as the Bodies to be electrified will contain only a certain Quantity of Electricity, of which more largely hereafter; when that Quantity is acquired, which is soonest done by a Number of Globes, the Surcharge is dissipated as fast as it is excited.

6. After the Globes had been a few times used, I found myself Master of a much greater Quantity of electrical Power, with much less Labour to myself, than when I used only Tubes. I could attract and repel light Substances at a much greater Distance than before; fire Spirits of Wine, Camphire, and all other Substances whose Vapours were inflammable, with great Ease, and at any Distance, with Non-electrics placed upon Originally-electrics: I could fire them, I say, at all times; though not equally easy, when the Weather was moist.

7. I discover'd with this Machine, and communicated to several Members of this *Society*, several of the Experiments said to be first made by M. *le Monnier* at *Paris*, before the Letter communicating them was received by our most worthy *President* from thence.

8. I order'd another Machine to be made for a Friend of mine, which carried a Globe of sixteen Inches Diameter. I united the Power of this large Globe with that of three of the others before-mention'd, and found the Strokes from the excited Non-electrics not increased according to my Expectation. In two Experiments indeed, where the Dissipation of the whole Power of these Globes was visible as fast as it was excited, the Effect of this additional

additional Globe was very confiderable. The firft was, when two Pewter Plates were held, one in the Hand of an electrified Man, and the other by one ftanding upon the Floor: When thefe Plates were brought near each other, the Flafhes of perfectly pure and bright Flame were fo large, and fucceeded each other fo faft, that, when the Room was dark-en'd, I could diftinctly fee the Faces of thirteen People, who ftood round the Room. The other was from a Piece of large blunt Wire hanging to the Gun barrel; from the End of which, when electrified, and any black * Non electric unexcited was brought near, though not near enough to caufe a Snap, a Brufh of blue lambent Flame, totally different from the former, was very confpicuous when the Room was dark, of more than an Inch long and an Inch thick. I mention that what is held near the Bottom of the Wire fhould be black, becaufe then you fee this Flame more fharp. Here the phofphoreal Smell might be perceived at a confiderable Diftance. If the Back of your Hand was brought fo near this Wire as to occafion a Snap, and thefe Snaps were received for fome time, you would feel them

* In the Courfe of thefe Obfervations, whenever I mention either Originally-electrics or Non-electrics, I always underftand the whole *Genus* of each. Thus when I mention a Man placed upon Originally-electrics, I am indifferent whether he is fufpended either in Lines of dry Silk, Hair, or Wool; or (which is much more convenient) if he ftands upon Glafs, Wax, Refin, Pitch, Sulphur, &c. or upon different Mixtures of thefe, if of a fufficient Thicknefs. As we are now Mafters of a greater electrical Power than heretofore, I have found the Electricity pervade, tho' in very fmall Quantity, Originally-electrics of above four Inches Diameter.

them like fo many Punctures upon your Skin, occafioning red Spots, which have lafted four and twenty Hours.

9. If, when a Perfon is electrified, he brings his Hand upon the Cloaths of one that is not, they both have a Senfation exactly refembling that of many Pins running into the Skin, which continues as long as the Globes are in Motion. This is moft perceptible when the Cloaths are of thin woollen Cloth or Silk, animal Subftances; lefs fo, when of Linen or Cotton, which are vegetable.

10. If fome Oil of Turpentine is fet on fire in any Veffel held in the Hand of an electrified Man, the thick Smoke that arifes therefrom receiv'd againft any Non-electric of a large Surface, held in the Hand of a fecond Man ftanding upon an electrical Cake; this Smoke, I fay, at a Foot Diftance from the Flame, will carry with it a fufficient Quantity of Electricity for the fecond Man to fire any inflammable Vapour. The electrical Strokes have been likewife perceptible upon the touching the fecond Man, when the Non-electric held in his Hand has been in the Smoke of the Oil of Turpentine between feven and eight Feet above the Flame. Here we find the Smoke of an Originally-electric a Conductor of Electricity.

11. Likewife if burning Spirit of Wine be fubftituted in the Place of Oil of Turpentine, and if the End of an iron Rod in the Hand of the fecond Man be held at the Top of the Flame, this fecond Man will kindle other warm Spirits held near his Finger. Here we find that Flame conducts the Electricity, and does not perceptibly diminifh its Force.

12. These two Experiments demonstrate, that the Opinion of those is erroneous, who suppose the electrical *Effluvia* to be of a sulphureous Nature; and that these themselves are set on fire at the Snapping observ'd, when you bring Non-electrics unexcited to those that are. If their Opinions were true, the electrical *Effluvia* should be destroyed by the Flame in both the preceding Experiments; the contrary of which is observed.

13. I now proceed to take notice of that surprising Effect, that extraordinary Accumulation of the electrical Power in a Phial of Water, first discover'd by Professor *Muschenbroek*, a Man born to penetrate into the deepest Mysteries of Philosophy: And I hope I shall stand excused, if I enter into a minute Detail of the Circumstances relating thereto. The Experiment is, that a Phial of Water is suspended to a Gun-barrel by a Wire let down a few Inches into the Water through the Cork; and this Gun-barrel, suspended in silk Lines, is applied so near an excited glass Globe, that some metallic Fringes inserted into the Gun-barrel touch the Globe in Motion. Under these Circumstances a Man grasps the Phial with one Hand, and touches the Gun-barrel with a Finger of the other. Upon which he receives a violent Shock through both his Arms, especially at his Elbows and Wrists, and across his Breast. This Experiment succeeds best, *cæteris paribus*,

1. When the Air is dry.

2. When the Phial containing the Water is of the thinnest Glass.

3. When the Outſide of the Phial is perfectly dry.

4. In proportion to the Number of Points of non-electric Contact. Thus if you hold the Phial only with your Thumb and Finger, the Snap is ſmall; larger when you apply another Finger, and increaſes in proportion to the Graſp of your whole Hand.

5. When the Water in the Phial is heated; which being then warmer than the circumambient Air, may not occaſion the condenſing the floating Vapour therein upon the Surface of the Glaſs.

14. From theſe Conſiderations it is to be obſerv'd, that this Effect ariſes from electrifying the non-electric Water, included in the originally-electric Glaſs; ſo that whatever tends to make the Outſide of the Glaſs non-electric by wetting it, as, a moiſt Hand, damp Air, or the Water from the Inſide of the Phial, defeats the Experiment, by preventing the requiſite Accumulation of the electrical Power.

15. That a Gun-barrel is abſolutely neceſſary to make this Experiment ſucceed, is imaginary; a ſolid Piece of Metal of any Form is equally uſeful. Nor have I yet found, that the Stroke is in proportion to the Quantity of electrified Matter; having obſerved the Stroke from a Sword as violent as that from a Gun-barrel with ſeveral excited iron Bars * in Contact with it.

16.

* If of ſix Men touching each other, and ſtanding upon Originally-electrics, one touches the Gun-barrel, the whole are electrified; all theſe then muſt be conſider'd, as ſo much excited non-electric Matter. From the Aggregate of all theſe, not more Fire is viſible upon the Touch than from either of them ſingly.

16. I have tried the Effect of increaſing the Quantity of Water in the Glaſſes of different Sizes, as high four Gallons, without in the leaſt increaſing the Stroke. If * Filings of Iron are ſubſtituted in the Room of Water, the Effect is conſiderably leſſen'd. If Mercury, much the ſame as Water; the Stroke is by no means increaſed in proportion to their ſpecific Gravities, as might have been imagined §.

17. The Phial ſhould not be leſs than can conveniently be graſped. I generally make uſe of thoſe, which hold ſeven or eight Ounces, and fill them about four Fifths with Water; and the Stroke from one of theſe, under the ſame Circumſtances, is equally ſtrong with that of a *Florence* Flask held in the Hand, which I have ſometimes made uſe of; though the Glaſs of this laſt is equally thin with that of the Phial, and the Quantity of Water four times as much. That the Stroke therefore is not as the Quantity of Water electrified, is evident from this Experiment. This Fact does not depend upon my Judgment alone, but likewiſe upon the Opinions of ſeveral learned Members of this *Society*, who have experienced the greater and leſs Quantity of Water.

18. If a dry Twig of Birch, or any other Wood, be run through the Cork inſtead of the metallic Wire,

* For a further Account of the Filings of Iron, made uſe of in this Experiment, ſee *theſe Tranſactions*, Vol. XLV. p. 197.

§ In this Experiment, and in others, wherein we aſſert, that the Stroke is not increaſed in proportion to the Quantity of electrified Matter; it muſt always be underſtood, that the excited Non-electrics themſelves are touched, without being contained in Originally electrics, as Water in the Glaſs; for otherwiſe (as will hereafter be ſpecified) the Effects of different Quantities of Matter will be very different.

Wire, the Stroke is not greater than is uſually felt from the Gun-barrel without the Application of the Water. The Stroke is likewiſe leſſen'd, if the Phial is held in the Hand with a Glove on.

19. After the Gun-barrel and Phial have been ſufficiently excited, which is done in a few Seconds, the Surcharge is diſſipated; ſo that the continuing the Motion of the Machine ever ſo long after the Saturation is complete, does not increaſe the electrical Force.

20. The Force of the Stroke from the electrified Phial does not increaſe in proportion to the Dimenſions of the Glaſs, or the Number of Globes employed. I have been ſtruck as forcibly with one Phial from a Globe of ſeven Inches Diameter, as when I made uſe of, at the ſame time, one of ſixteen Inches, and three of ten. I have been lately informed, that at *Hamburgh* a Sphere was employed for this Purpoſe a *Flemiſh* Ell in Diameter, without the expected Increaſe of Power.

21. When the Phial is well electrified, and you apply your Hand thereto, you ſee the Fire flaſhes from the Outſide of the Glaſs wherever you touch it, and crackles in your Hand.

22. The Phial may be electrified by applying the Wire therein to the Globe in Motion; after which, if it is graſped in one Hand, and the Wire touched with a Finger of the other, the Stroke is as great as from the Gun-barrel. If you only bring your Finger near the End of the Wire without touching it, you obſerve the ſame Bruſh of blue Flame, as from the Wire hanging to the Gun-barrel, before taken notice of. This inſtantly diſappears upon touching

touching the Wire, though you do not receive a Shock, unless at the same time you grasp the Phial.

23. If you grasp the Phial with your Hand, and do not at the same time touch the Wire, the acquired Electricity of the Water is not diminished. So that, unless by Accident or otherwise the Wire is touched, the electrified Water will contain its Force many Hours, may be convey'd several Miles, and afterwards exert its Force upon touching the Wire.

24. If, when the Machine is in Motion, the Phial is hung upon the Gun-barrel, no Increase of the Stroke is perceived upon touching the Gun-barrel with your Finger, unless at the same time the Phial is taken in the Hand.

25. If, when the Gun-barrel and Phial are excited, you grasp the Phial with one Hand, and touch the Gun-barrel with a Piece of any Metal held in the other, the Shock is as great in your Arms as though you touched the Gun-barrel with your Finger; but not the least Shock is felt, if, instead of Metal, you touch the Gun-barrel with a Piece of dry Wood.

26. I have felt a very great Stroke, when I hung two Phials to the Gun-barrel, and, grasping them both, brought my Forehead near it. The Shock then was so violent, that I seem'd stunn'd, as though struck on the Head with a great Stick, and I have never since chose to repeat this Experiment. This Increase of the electrical Force was owing to the additional Phial, whereby the Points of non-electric Contact were augmented.

27. Likewiſe if a Perſon placed upon Originally-electrics, graſps two Phials, as before-mention'd, and a ſecond Perſon, ſtanding upon the Floor, touches any Part of his Body, a very ſlight Stroke only is perceived. But if the ſecond Perſon, while the Globes are in Motion, places one of his Fingers upon the Hand, or any Part of the naked Body of the firſt, and at the ſame time touches the Gun-barrel with his other Hand; both feel a Shock equal to that juſt now mention'd, but more tolerable, becauſe not felt in the Head, in the Arms only, and acroſs the Breaſt. In this Experiment, it is not neceſſary that the Outſide of the Glaſſes held in the Hands ſhould be dry, as in the former Experiments; becauſe whatever by the Moiſture is communicated to the Man, is ſtopped by the Originally-electrics upon which he is placed. If, inſtead of his Hand, you gently touch the firſt Perſon's Cloaths, you only perceive a ſmall Stroke upon your Finger; but if you preſs his Cloaths cloſe to his Body, you frequently perceive a double Stroke; the one, ſlight from his Cloaths; the ſecond, a violent Shock from his Body.

28. Upon ſhewing ſome Experiments to Dr. *Bevis*, to prove my Aſſertion that the Stroke was, *cæteris paribus*, as the Points of Contact of Non-electrics to the Glaſs, that ingenious Gentleman has very clearly demonſtrated it likewiſe by the following Experiment: He wrapped up two large round-bellied Phials in very thin Lead ſo cloſe as to touch the Glaſſes every-where, except their Necks. Theſe were filled with Water, and cork'd, with a Staple of ſmall Wire running through each Cork into the Water.

Water. A Piece of ſtrong Wire about 5 Inches long, with an Eye at each End, was provided, and at each End of this hung one of the Phials of Water by the ſmall Staple running through the Cork. A ſmall Wire Loop then was faſten'd into the Lead at the Bottom of each Phial, and into theſe Loops was inſerted a Piece of ſtrong Wire like the former. If then theſe Phials were hung acroſs the Gun-barrel and electrified, and a Perſon ſtanding upon the Floor touched the bottom Wire with one Hand, and the Gun-barrel with the other, he received a moſt violent Shock through both his Arms, and acroſs his Breaſt.

29. Theſe Phials may be concealed, and the Shock be more univerſal, in the following manner: The Phials may be placed in a Corner of the Room, and any thing laid over them, ſo as not to touch the upper Wires; then a very fine Wire muſt be ſuſpended to the Gun-barrel, and faſten'd to the upper ſtrong Wire. A ſecond Piece of ſmall Wire, of a ſufficient Length to reach from the Phials almoſt under the Gun-barrel, muſt be faſtened to the lower ſtrong Wire, and this may be conceal'd under a Floor-cloth. The Phials then are electrified; and if a Perſon, placing his Foot upon the Floor-cloth over the Wire which comes from the Bottom of the Phials, touches the Gun barrel, he receives a moſt terrible Shock. The firſt time I experienced it, was when the Phials were fully electrified, and both my Feet were placed upon the Wire. Upon receiving the Stroke from the Gun-barrel upon my Finger, it ſeemed to me, uſed as I am to theſe Trials, as though my Arm were ſtruck off at my Shoulder,

Shoulder, Elbow, and Wrift; and both my Legs, at the Knees, and behind near the Ankles. So that, to try the Effects of this Experiment, you muft be careful of not electrifying the Phials too much. If a dozen or more of thefe Phials, or one very large Bottle, were cover'd over with thin Lead in the above manner, and ftrongly electrified, and this Electricity were difcharged by a Man at once in the manner here mention'd, I fhould dread the Confequences.

30. We muft obferve, that this Shock is not felt, unlefs the Wire, coming from the Bottoms of the Bottles, is touched; and then not, if the Shoes are dry, and of confequence originally-electric. In this Experiment we fee the Effects of the Increafe of the Points of Contact; and it feems the more furprifing to thofe who are not acquainted with the Caufe, when the Wire is concealed under a Floor-cloth, that the moving of their Feet only one Inch, fhould occafion them, all other Circumftances apparently the fame, to feel a violent Shock, or none at all. A thick Carpet, inftead of a Floor-cloth, is liable to prevent the Succefs of this Experiment, for the fame Reafon as dry Shoes. This Experiment may aptly enough be called, the fpringing an electrical Mine.

31. If, in the former Experiment, the lower fmall Wire is faften'd to an iron Rod; and if, when the Phials are ever fo ftrongly excited, that Rod is held in the Hand of a Man ftanding upon the Floor, and with it he touches the Gun-barrel, he perceives no Shock, for Reafons prefently to be affigned. But if he takes this iron Rod in one Hand, and touches the Gun-barrel with the other, he then is violently

ſtruck. We muſt here obſerve, that the Violence of the Stroke is always felt in our Bodies, in proportion to the Loudneſs of the Exploſion, and the Quantity of Fire ſeen. Therefore, as both theſe are equally perceptible, whether the Electricity paſſes only thro' the Iron, as in the firſt of theſe Inſtances, or thro' our Bodies equally with the Iron, as in the ſecond; we conclude, that in both there is in the ſame Degree of electrical Force. By the firſt of theſe Methods you are capable of making others ſenſible of the electrical Force, without feeling it yourſelf. This Experiment, as well as the laſt, will admit of infinite Variation.

32. If a Man, ſtanding upon an electrical Cake, takes the Phial ſuſpended to the Gun-barrel in his Hand, by theſe means he acquires ſome electrical Power; for if, under theſe Circumſtances, he touches the Gun-barrel, he only receives a ſlight Stroke. If then, without having had any Communication with unexcited Non-electrics, he touches the Gun-barrel again, the Globes being yet in Motion, he receives no Stroke at all.

33. If to the Gun-barrel an Egg, either raw or boiled, is ſuſpended by a Piece of Wire, and a Perſon, graſping the electrified Phial in one Hand, brings the Palm of his other near the Bottom of the Egg; at that Inſtant he receives a ſmart Stroke, and his Hand ſeems full of a more red Fire than is uſually obſerved. In this Experiment the Stroke is more confined to the Hand without ſhocking the Arms, than when you touch the Gun barrel itſelf; it more reſembles a Stroke over the Hand with a *Ferula*.

34.

34. If any Number of People ſtand upon Originally-electrics, and communicate with each other by any non-electric Medium, eſpecially Metal, they are by theſe means all equally electrified; and if a Perſon ſtanding upon the Floor, and holding the Phial of Water hanging to the Gun-barrel in his Hand, touches the Perſon furtheſt from the Gun-barrel, the whole Number receives a Shock equal to any one touching the Gun-barrel ſingly.

35. If a Number of Perſons, how great ſoever, ſtand upon the Ground, communicating with each other as before, the firſt of which graſps the Phial, and the laſt touches the Gun-barrel, the whole Number receive a Shock like the former. This, we are inform'd, M. *le Monnier* at *Paris* communicated through a Line of Men, and other Non-electrics, meaſuring nine hundred Toiſes.

36. Several Experiments ſhew, that the electrical Force always deſcribes a Circuit; *e. g.* if a Man holds the electrified Phial in one Hand, and touches the Gun-barrel with the other, he feels the Shock in no other Parts of his Body than in his Arms, and acroſs his Breaſt. So that here we ſee the electrical Power darts *rectiſſimo curſu* between the Gun-barrel and Phial. This is more particularly demonſtrated by the following Experiment, in which, though the two Lines of Perſons may be of any Length, we only ſpecify, that each conſiſts of four, for the ſake of Perſpicuity.

37. Of one Line, let *A* (*ſee* TAB. III.) touch the Gun-barrel, ſtanding upon Wax, and communicate with *BCD* likewiſe ſtanding upon Wax. Of the other Line, let 1 take the electrified Phial in his Hand,

Hand, and join with 2, 3, and 4, all ſtanding upon the Floor. If, under theſe Circumſtances, the firſt Line is electrified, and 4 touches *D*, all eight are ſtruck through. If 4 touches *C*, *D*, though electrified, feels nothing, and the remaining ſeven are ſtruck; ſo that here *D* is left out of the Circuit. If 4 touches *B*, only ſix feel the Shock, and *C* and *D* feel nothing; and thus you may proceed to *A*, who muſt always neceſſarily feel, if either himſelf or any of his Line is touched. If, when both Lines are as before-mention'd, *D* touches 3, 4 is left out of the Circuit, and the remaining ſeven feel the Stroke. If *C* touches 2, the Circuit conſiſts of five, *D*, 3, and 4 being, though under the ſame Circumſtances, left out: Always obſerving, however theſe Circuits are diverſified, that *A*, who touches the Gun-barrel, and 1, who holds the Phial, are certain to feel the Stroke.

38. This Experiment may be reverſed, the Lines being as before, in the following manner, wherein likewiſe this Circuit is always obſervable. Let *A* (*ſee* TAB. III.) touch the Gun-barrel as before, and *D* hold the Wire of the electrified Phial in his Finger. Let 4 graſp the Phial, and 1 touch *B*; then *A* feels nothing, being left out of the Circuit, and the other ſeven are ſtruck. If 4 touches *C*, then *A* and *B* feel nothing, the Circuit conſiſting of the remaining ſix. But it is to be obſerved, as in the former Experiment, that 4, who graſps the Phial, and *D*, who holds the Wire, muſt of Neceſſity be always in the Circuit. I have been the more particular in this Matter, as it demonſtrates the Courſe of

Aaaaa the

the electrical Power to be in the most direct manner between the Gun-barrel and the electrified Phial.

39. Likewise, if a Person, standing upon an Originally-electric, touches the Gun-barrel with his right Hand, a Piece of Wire being placed round his left Leg, and a second Person, standing likewise upon the Wax, takes hold of the Extremity of this Wire; then let another Person, standing upon the Floor, and grasping the electrified Phial, touch any Part of the second Person's Body. Upon this Touch, the second Person is shook as usual; but the first feels the Stroke only in his left Leg and right Arm, the nearest Course of the electrical Power.

40. If any Number of Persons communicate by Pieces of Wire, and if any one of them brings together the Ends of the two Pieces of Wire in his Hands, upon the Gun-barrel's being touch'd, he will perceive no Stroke. But if the Ends of the Wires are but a Quarter of an Inch asunder, he will be shook in both his Arms; because then his Body will become Part of the Circuit.

41. If, when any Number of Persons join Hands, or communicate by any metallic Medium standing on the Floor, one grasps the Phial, and joins with the rest; upon the Gun-barrel's being touch'd by the last Person of the Line, the whole Number are struck, and he who grasps the Phial, as forcibly as the rest. But if two Phials are employed, and he grasps them both, with a Piece of Wire of sufficient Length held between his Fingers, which Wire touches both Phials, and its End is taken hold of by the second Person of the Line; if then the last Person touches the excited Gun-barrel, all in the Line

Line are violently ſtruck, except the Perſon who graſps the Phials; but he feels little or nothing of the Stroke.

42. The Stroke is very violent, when a Wire is put round the naked Head, or under the Peruke, and the Perſon graſping the Phial touches the Gun-barrel with the Ends of the Wire, or if he holds the Wire between his Teeth.

43. If a Perſon, ſtanding on the electrical Cakes with Gold or Silver Lace upon his Coat, takes hold of the Gun-barrel, and another Perſon graſping the electrified Phial touches the Bottom of the Lace, the Perſon electrified, if he holds down his Head, feels the Blow under his Chin. The Lace in this Inſtance has the ſame Effects as a Piece of Metal; at the End of which, if placed in the ſame manner, you would neceſſarily feel the Stroke.

44. I now proceed to ſhew, by what Steps, in my Inquiries into the Nature of Electricity, I diſcover'd that the glaſs Tubes and Globes had not the electrical Power in themſelves, but only ſerved as the firſt Movers and Determiners of that Power.

45. Several Months ſince, I obſerv'd that, by rubbing a glaſs Tube, while ſtanding upon a Cake of Wax, in order, as I expected, to prevent any of the electrical Power from diſcharging itſelf through me into the Floor; contrary to my Expectation, that Power was ſo much leſſen'd, that no Snapping was to be obſerv'd upon another's touching any Part of my Body. But if a Perſon not electrified held his Hand near the Tube whilſt it was rubbing, the Snapping was very ſenſible. This I ſhew'd to ſeveral Members of the *Royal Society*,

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and other, who did me the Honour to visit me. Afterwards I met with an Experiment of the same kind, in a Treatise publish'd by Professor *Bose*, intitled, *Recherches sur la cause et sur la veritable theorie de l'Electricite*, which that ingenious Gentleman says, had given him great Trouble by its Oddness. The Experiment is, that, if the electrical Machine is placed upon Originally electrics, the Man who rubs the Globes with his Hands, even under these apparently favourable Circumstances, gives no Sign of being electrified, when touched by an unexcited Non-electric. But if another Person, standing upon the Floor, does but touch the Globe in Motion with the End of one of his Fingers, or any other Non electric, the Person rubbing is instantly electrified, and that very strongly. The Solution of this *Phænomenon*, seemingly contrary to the already discover'd Laws of Electricity, had terribly tormented him; but however he has given us the following, which he modestly calls a plausible Subterfuge rather than a Solution; *viz.* that a Power cannot act at the same time with all its Vigour, when one Part of it is already employed; as a Horse, who already draws an hundred Pounds, cannot draw an additional Weight as freely as if he had not been loaded at all. That the Hand excites the Virtue already in the Sphere; therefore if the same Power impregnates the Man, there remains none for the Globe. That the Virtue of the Globe then cannot be communicated at the same time to the Man, by whom it is created. That he, who gives it, cannot receive it himself. From these, and such-like Considerations, it appears to him,

him, that the Man upon the Ground, who holds his Fingers to the Globe in Motion, instead of his diminishing its electrical Force, throws that Force back again over the Man, who excited it. That the Finger in this Case seems to operate as an Electric *per se*, and drives back the electrical Power.

46. I have seen an Account of * Mr. *Allamand*, lately printed at the *Hague*; wherein he takes notice of this *Phænomenon*. He tells us, that as Part of the electrical Power of the Globe passes off by the Frame, upon which the Globes are mounted, into the Floor, and dissipated thereby; he conceived, that if the Machine, and the Man who rubb'd the Globe, were placed upon Pitch, to prevent this Dissipation, the Fire of Electricity would be more strong. But the Consequence is extremely odd and unexpected; for the contrary happens; and the electrical Power is considerably diminished, and sometimes there is even none at all.

47. I tried this Experiment several times with my Machine, and the Man, who turns the Wheel thereof, mounted upon the electrical Cakes. If the Air was dry, and the Machine placed at some Distance from non-electrical Substances, as the Sides of the Room, Chairs, and such-like; after one or two small Snaps, the Gun-barrel, supported by silk Lines, and hanging in Contact with the Globes, would, tho' the Machine were in Motion a considerable time, attract

* *Bibliotheque Britannique pour les Mois de Janvier, Fevrier, et Mars*, 1747.

attract no light Subſtances, nor emit any Fire. This induced me to conceive, that the electrical Power was not inherent in the Glaſs, but came from the Floor of the Room; and if the Fact were ſo, the Gun-barrel ſhould ſnap upon my touching any Part of the Machine. The Conſequence fully anſwer'd my Conjectures; for while I ſtood upon the Floor, the Globes ſtill in Motion, I put one Hand upon the Frame of the Machine, and touched the Gun-barrel with one of the Fingers of my other. Upon this, Fire iſſued, and the Snapping continued as long as I held my Hand upon the Machine, but ceaſed upon taking it off. This at once proved to me, that the electrical Fire paſſed from the Floor thro' my Body to the Machine. I then order'd the Man to put one of his Feet from the Wax upon the Floor; which, as ſoon as he complied with, cauſed the Electricity to ſnap at the Gun-barrel, and this ceaſed upon his replacing his Foot. Here I found, that the electrical Power came through the Man; and that, in theſe Inſtances, either myſelf, or the Man who touched the Floor with his Foot, was to be regarded as an additional Part of the Machine communicating with the Floor. Theſe Conſiderations led me to make the following Experiments.

[illegible]. If my Conjectures were well founded, and if the electrical Power, the Man and the Machine being placed upon Originally-electrics, went through my Body to the Machine, a fine Wire, held in my Hand at a few Inches Diſtance, ought to be attracted by any Part of the Machine. This ſucceeded accordingly, but the Attraction laſted a very ſmall

Space

Space of Time, and the Wire again hung perpendicularly from my Finger, though the Globes continued in Motion. This induced me to believe, that the Gun-barrel, and the other Non-electrics suspended in Contact with the Globes, would only contain a certain Quantity of the electrical Æther; and if this were the Case, the Attraction of the Wire to the Machine would be continual, if the electrical Power found again a Communication with the Floor, as the Wire was the only Canal of Communication between the Floor and the Machine. Whereupon I placed one of my Fingers upon the Gun-barrel, and held a Wire near the Machine with my other Hand, and found, that as long as my Finger continued upon the Gun-barrel, the Wire was attracted, but no longer.

49. Here we find, that one Cause of the electrical Attraction is the Current of the electrical Æther setting to the Machine through the Wire; and this Current is stopped from two Causes; one, when there is no Discharge thereof from the Gun-barrel, the Accumulation being complete; the other, when other Currents are opened, that is, when the Machine is touched in other Parts.

50. In these, and the subsequent Experiments, I always suppose the Air very dry; for if it is not, and the silk Lines, which support the Non-electrics, are wetted thereby, the electrical Power will be discharged along them, and the Wire will be constantly attracted, as I have frequently on purpose experienced; and this Discharge is in proportion as the Lines are more or less wetted.

51. If a Man ſtands upon the Machine placed upon Originally-electrics, and the Gun-barrel with the other Non-electrics are ſuſpended as uſual in Contact with the Globes, no Electricity is obſerved in that Man: But if a Wire, hanging to the Wainſcot of the Room, touches the Gun barrel, or a Man ſtanding upon the Floor applies his Finger thereto, the Man upon the Machine emits Fire copiouſly; and either himſelf, or the Man who turns the Wheel of the Machine, fires inflammable Subſtances. But this Effect is no longer obſervable, when the Wire, &c. are removed from touching the Gun-barrel. So that, in this Experiment, the uſual Courſe of the Electricity is inverted; and that Power, which, in moſt other Inſtances, is brought by the Wood-work of the Machine to the Globes, and by them diſcharged upon the Gun-barrel, is now brought by the Wire to the Gun-barrel, and from this the Globes throw it all over, not only the Machine, but any Non-electric in Contact with it, if the Electricity is ſtopp'd. In this Experiment, if an iron Rod, ſtanding upon the Floor, is inclined againſt the Loops of the ſilk Lines which ſupport the Gun-barrel, in ſuch a manner as not to touch the Gun-barrel, the electrical Fire, which paſſes from the iron Rod to the Gun-barrel, inſtead of being ſupplied conſtantly, comes in by ſnapping ſo long as any unexcited Non-electric communicates with the Machine, but ceaſes upon its being removed: And if the Air is very dry, and none of the Electricity conducted down the ſilk Lines, the Snapping from the iron Rod to the Gun-barrel will frequently correſpond to the touching of the wooden Machine with

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your Fingers, and ſtop upon your taking them off. And this Experiment will look much like Magic, even to thoſe who are acquainted with the Operations of Electricity; for if the Perſon who turns the Wheel of the Machine, and ſtands upon the Cakes, be properly inſtructed: upon your bidding the Gun-barrel ſnap, he only puts the Toe of his Shoe upon the Floor, and it ſnaps immediately, and continues ſnapping as long as he keeps it there; but if you order it to ceaſe ſnapping, he almoſt imperceptibly replaces his Foot upon the Cakes, and it ceaſes. This may be repeated as often and as long as you pleaſe.

52. Many Experiments demonſtrate, that if the Electricity is not ſtopt, no Sign of its Preſence, either by Fire or Attraction, is obſervable in the non electric Bodies ſuſpended to the Globes: that is, although ever ſo great a Quantity be determined by the Globes over theſe Bodies, the Electricity paſſes off from them *pleno rivo* to the Floor, from whence it came: but if the Electricity is ſtopt, it is then accumulated upon theſe Non-electrics; but this can be done only to a certain Degree, as is manifeſt from a former Experiment. And if, when this Power is accumulated, a Man ſtanding upon the Floor touches now-and-then the Non-electrics with his Finger, the Electricity, which is here accumulated, ſnaps, and the Fire is always obſervable. But this Snapping is not, when the electrical Power paſſes off continually, as from a Piece of blunt Wire hung to the ſuſpended Gun-barrel, and the Hand of a Man brought near it without touching; whereby the electrical Power becomes viſible, like a fine

blue Cone of Flame, with its Point towards the Wire. When the Hand is placed at a proper Distance, the Blast, like that of cold Air, is therefrom very manifest. If you do not determine the Electricity by these means to a Point, the Dissipation of it is general, and from all Parts of the excited Non-electric; but if you do, by bringing your Hand near the Wire as before-mention'd, you see the Manner of its being discharged into the Floor, and so into the Earth. These Facts being so, if my Conceptions are true, that the glass Globes circulate the electrical Fire, which they receive from their Friction against the Cushions, or the Hand of a Man, and which is constantly supplied to these last from the Floor; the Ingress of the electrical Fire, if the Machine, *&c.* are placed upon Electrics *per se*, ought to be visible, as well as the Egress under the same Circumstances; and this is demonstrated by Experiment, For if, while any unexcited Non-electrics touch the Gun-barrel, the Globes being in Motion, you bring your Finger, or a Piece of Wire near any Part of the Wood-Work of the Machine, but more especially the iron Axis of the Wheel; you observe the Brush of blue Flame set in from it to the Wood-Work. We always observe, in this Experiment, that the lambent Flame from the End of the Wire passes diverging into the Machine, and this continues so long as the Gun-barrel is touch'd. So that here the Office of the Globes exactly tallies with that of the Heart in Animals; which, as long as the Quantity of Blood is supplied, propels it into the Arteries, and these all over the System; or that of the Pump in Hydrostatics. In the same manner, by

by the Attrition of glaſs Tubes, the electrical Power is brought from the Body of the Man who rubs the Tube; and he is conſtantly taking in a Supply from the Floor.

53. What I here call the electrical Æther, is that Atmoſphere which ſurrounds both excited Originally electrics, and excited Non-electrics. That this is extended to a conſiderable Diſtance, appears, from a fine Thread, or Piece of Cotton-graſs Seed, being attracted at ſome Diſtance from them, as far as which, it is preſumed, this Atmoſphere extends. Here indeed it is only perceived by its Effects upon theſe light Subſtances: but at the Bruſh of Flame from the End of the Wire before-mention'd, from ſome Bran lying upon a flat Piece of Metal in Contact with excited Non-electrics, your Hand being held over it, and in many other Experiments, it becomes manifeſt to your Feeling as a Blaſt of cold Wind. You feel it likewiſe in a leſs Degree, when a glaſs Tube is well excited, and brought near your Face. If no unexcited Non-electric is near, this Atmoſphere ſeems to be determined equally over all the excited Non-electrics in Contact with the Machine; but if a Non-electric unexcited is brought near, the greateſt Part of it is determin'd that way; and hereby the Attraction at any other Part of theſe excited Non-electrics is conſiderably diminiſhed. Hence the Cauſe of the Repulſion of Electricity, which does not operate, until the electrical Æther is ſufficiently accumulated. This electrical Repulſion is ſtrongeſt in thoſe Parts of the excited Non-electrics, where unexcited Non-electrics are brought near them; for by theſe the electrical Blaſt, which

 otherwiſe,

otherwiſe is general, is particularly determined to the Floor.

54. Before I proceed further, I muſt beg Leave to explain what I call the Accumulation of Electricity. To put a ſimilar Caſe: As we take it for granted, that there is always a determinate Quantity of Atmoſphere ſurrounding the terraqueous Globe, we conceive, when we ſee the Mercury in the Barometer very low, that there then is a leſs accumulated Column of this Atmoſphere impending over us, than when we ſee the Mercury high. In like manner when we obſerve that the electrified Gun-barrel attracts or repels only very light Subſtances at a very ſmall Diſtance, or that the Snap and Fire therefrom are ſcarcely perceptible; we conceive then a much leſs Quantity of electrical Atmoſphere ſurrounding the Gun-barrel. This Power being more or leſs, we call the greater or leſs Degree of the Accumulation of Electricity. This is only attainable to a certain Point, if you electrify ever ſo long; after which, unleſs otherwiſe directed, the Diſſipation thereof is general. The Phial of Water of *Muſchenbroek* ſeems capable of a greater Degree of Accumulation of Electricity, than any thing we are at preſent acquainted with: And we ſee, when, by holding the Wire thereof to the Globe in Motion, the Accumulation being complete, that the Surcharge runs off from the Point of the Wire, as a Bruſh of blue Flame. A Method has been diſcover'd here by a Gentleman (Mr. *Canton*) by which the Quantity of accumulated Electricity may be meaſured to great Exactneſs. The Manner of meaſuring is this: When the Phial is ſufficiently electrified by applying

applying the Wire thereof to the glaſs Globe, and which is known by the Appearance of the Bruſh of Flame at the End of the Wire, as before-mention'd; hang a ſlender Piece of Wire to the ſuſpended Gun-barrel for this Purpoſe detached from the Globes. Upon your applying the Wire of the electrified Phial to that hanging to the Gun-barrel, you perceive a ſmall Snap; this you diſcharge by touching the Gun-barrel with your Finger, which likewiſe ſnaps: And thus alternately electrifying and diſcharging, you proceed until the whole Electricity of the Water is diſſipated; which ſometimes is not done, under an hundred Diſcharges. If you do not diſcharge the Electricity every time, the Snaps from the Wire of the electrified Phial to the Gun-barrel are ſcarcely perceptible. In proportion to the Number of Strokes, you eſtimate the Quantity of the acquired Electricity of the Water. That you could, by ſtopping the Electricity, excite Non-electrics; and, by accumulating their Power, make them exert more Force than Originally-electrics would at any Point of Time, was that capital Diſcovery of the late Mr. *Gray*; and is to be regarded as the Baſis, upon which all the preſent Improvements of our Knowledge in Electricity are founded; and till which Diſcovery, although ſome of the Effects of Electricity were obſerved above two thouſand Years ago *, little Progreſs was made.

55.

* *Theophraſtus*, who lived above three hundred Years before the Date of the Chriſtian Æra, takes Notice of Amber and the *Lyncurium*,

55. The electrical Æther is much more subtil than common Air, and passes to a certain Depth through all known Bodies. It passes most readily through Metals, Water, and all Fluids, except resinous ones; then animal Bodies dead or alive, in proportion as they are more or less wet; then Stones, Wood, and Earths. It passes to a certain Thickness only thro' Resins, dry animal Substances, Wax, and Glass. For this Reason Bodies are called Electrics *per se*, or Non-electrics; not only for their rubbing the Electricity from other Bodies, but likewise as they permit more or less of the electrical Æther to pass through them. This Æther has not only the Property with Air of moving light Substances; but it seems to have another, and that is Elasticity.

56. That this Fluid is more subtil than common Air, is more particularly demonstrated by its passing through several Glasses at the same time; through any one of which, though ever so thin, Air cannot pass. It likewise passes, as I have mention'd before, through all known Bodies, except Originally-electrics, and even through these to a certain Degree. Its Elasticity is proved by its extending itself round excited Electrics, and excited Non electrics, to a considerable Distance; as well as by its increasing the Motion of Fluids. This is demonstrated by the Experiment with a small glass Siphon where the

curium attracting not only Straws, and Shavings of Wood, but also thin Pieces of Copper and Iron. See *Theophrastus* περὶ τῶν λίθων ν'. —Καὶ τὸ λυγκούριον — ἕλκει γὰρ ὥσπερ τὸ ἤλεκτρον· Οἱ δέ φασιν οὐ μόνον κάρφη καὶ ξύλον, ἀλλὰ χαλκὸν καὶ σίδηρον, ἐὰν ᾖ λεπτός· ὥσπερ καὶ Διοκλῆς ἔλεγεν. See *p.* 74. in the late Edit. by *J. Hill.*

the Elaſticity of the electrical Æther overcomes the Attraction of Coheſion: I have frequently obſerved this Experiment does not operate, unleſs the greateſt Part, if not the whole electrical Blaſt, is determined to the Floor through the Water, by bringing ſome unexcited Non electric near the long Leg of the Siphon †. The Stream through this ſlender Tube is moſt complete, when the Non-electric is brought near, ſo as when the Room is ſomewhat darkened, the Stream of Water appears as a Stream of blue Flame, much like that from the blunt Wire. This Stream is ſtopped, either by touching any Part of the Non-electrics in Contact with the Globes; by placeing the Machine and the Man who turns the Wheel upon Electrics *per ſe*, by which the Current of the electrical Æther from the Floor to the Machine is prevented; or by removing the Non-electric from the Leg of the Siphon, by which the Diſſipation of the electrical Æther from the excited Non-electric becomes general. So that we find, that although we can repel light Bodies from many Parts of excited Non-electrics at the ſame time; the whole Force of the electrical Current is neceſſary, to drive off

† There is one Inſtance, where the Water will run off in a full Stream without bringing a non-electric unexcited near the long Leg of the Siphon; and that is, by ſuſpending a Phial of Water, as uſual to the Gun-barrel by a Wire, and by letting a glaſs Siphon through the Cork into the Water. When this Phial is ſufficiently electrified, the Water therein runs off in a full Stream, though no Non-electric unexcited is near; becauſe then the Current of Water through the Siphon is the only Way, by which the Surcharge of the Electricity can be diſſipated.

off so ponderous a Fluid as Water. May we likewise not infer the Elasticity of electrical Æther, from the Ingress of the blue Flame from the End of a blunt Wire held near the Axis of the Wheel, or any Part of the Wood work of the Machine, after the Revolutions of the Globes are ceased? Certainly we see an Influx of electrical Fire to all Bodies, until their determined Quantity is restored. Is not the Elasticity of this Æther deducible likewise from the violent Shock we feel in our Bodies in the Experiments with Water?

57. There seems to be a Quantity of this Æther in all Bodies. Hence the Reason why, though the Machine is placed upon Electrics *per se*, a Snap or two, as I mention'd before, is observ'd upon touching the Gun-barrel, when the Machine has been some time in Motion: But after these no more is perceiv'd, if the silk Lines are very dry, and the electrical Supporters of the Machine are of a requisite Thickness. As soon as any Non-electric unexcited touches the Machine, this Loss is immediately restored. As the electrical Æther, as has been specified, is an elastic Fluid, wherever there is an Accumulation thereof, there is an Endeavour by the nearest unexcited Non-electric to restore the *Æquilibrium*. The restoring of this *Æquilibrium* I take to be the Cause of the Attraction of excited glass Tubes and Globes, as well as that of excited Non-electrics; for here the Blast of electrical Æther constantly sets in from the nearest unexcited Non-electrics towards those excited, and carries with it whatever light Bodies lie in its Course. This setting in of

of the Current of electrical Æther towards excited Non-electrics is likewise very perceptible to your Feeling as a Blast of cold Wind; if when you are electrified, you hold your Hand over a Plate with some Bran in it, by which Blast the Bran is carried against your Hand. These light Substances are again repell'd by the Blast from the excited Bodies, as soon as they come in Contact, and sometimes before. The Successions of these alternate Attractions and Repulsions are extremely quick, so that sometimes your Eye can hardly keep Pace with them. And if you put a glass Globe of about an Inch in Diameter very light and finely blown into a Plate of Metal, and hang another Plate over it; electrify the upper one, and bring the other under it, and you will find the Strokes from the alternate Attractions and Repulsions * almost too quick for your Ear. I have seen a *German*, who travell'd with a small electrifying Machine, who, by a Process of this sort, made two small Bells ring. One of the Bells was suspended to an electrified Wire, which was conducted without touching along the Sides of the Room; at about an Inch Distance, detached from this Wire, a little Clapper was hung by a silk Line; at an equal Distance from this last was hung another little Bell, which communicated with the Sides of the Room.

As

* The following is an Argument of the Velocity likewise, with which these little Globes are attracted and repell'd. If they are let fall from the Height of six Feet or more upon a wooden Floor, or a Plate of Metal, they are rarely broke; but by the Attractions and Repulsions of them between the Plates, though at the Distance only of one sixth of an Inch, they are frequently beat in Pieces.

As ſoon as the Machine was in Motion, the electrified Bell attracted the Clapper, which immediately by the repulſive Blaſt was blown off to the unexcited Bell. By the time the ſecond Bell was ſtruck, the former attracted again; and this Jingling of the two Bells continued not only during the Motion of the Machine, but ſeveral Seconds after it was ſtopped. This was occaſioned by the ſmall Volume of the Clapper being able to convey away only a ſmall Quantity of the electrical Æther at each Stroke; by which it was ſome time before the *Æquilibrium* was reſtored.

58. To demonſtrate likewiſe, that the reſtoring this *Æquilibrium* is not imaginary, I ſhall mention an Experiment of a Gentleman (Mr. *Wilſon*) who has taken great Pains in theſe Inquiries. Take two Plates of any Metal, very clean and dry, whoſe Surfaces are nearly equal; hang one of them to any excited Non-electric, and bring under it upon the other a whole Leaf of Silver. When, which you find upon Application, the ſilver Leaf is attracted, lower the bottom Plate; if it is too low, you will obſerve the leaf Silver jump up and down; if too high, it will only be attracted in Part, and thereby diſſipate the electrical Power. But if you get it at the proper Diſtance, which will very eaſily be found upon Trial, the Silver will be perfectly ſuſpended at right Angles with their Planes, like the *Trapezium* of the Geometers, and touch neither of the Plates; it will be extended likewiſe to its utmoſt Dimenſions. You frequently obſerve, both at the Top and Bottom of the Silver, the electrical Fire. The ſame Effect is produced, if you reverſe the Experiment,

ment, by electrifying the bottom Plate, and suspending the other over it. Now I conceive, that the Space occupied by this Leaf of Silver, is that where the *Æquilibrium* of the electrical Æther is restored; for if you take away the under Plate, thro' which from the Floor the Flux of this Æther is furnished, or if that Plate be placed upon an Electric *per se*, by which this Flux is prevented likewise, the silver Leaf is blown away.

59. No Body can be suspended in *Æquilibrio* but from the joint Action of two different Directions of Power: So here, the Blast of electrical Æther from the excited Plate blows the Silver towards the Plate unexcited. This last, in its Turn, by the Blast of electrical Æther from the Floor setting through it, drives the Silver towards the Plate electrified. We find from hence likewise, that the Draught of electrical Æther from the Floor, is always in proportion to the Quantity thrown by the Globes over the Gun-barrel; or the *Æquilibrium* by which the Silver is suspended, could not be maintained. I once found, that a Gentleman, at that time an Invalid, whose Shoes were perfectly dry, and of consequence Originally-electrics, and who was employ'd to hold the Non-electric Plate through which the Æther was to come from the Floor; this Gentleman, I say, did not furnish a sufficient Quantity, because of the Dryness of his Shoes, to maintain the *Æquilibrium*; and the Silver was blown away. But upon employing another to this Office, whose Shoes were more wet, the Æther came readily through him, and the Silver was suspended. I have likewise found a wooden Pole, very dry, not

conduct this Æther fast enough to keep the Silver suspended. It may be imagined, that it is possible for the Silver to be suspended, without supposing a Flux of the electrical Æther from the nearest unexcited Non-electric, as well as from the excited one; that is, by the simple electrical Attraction. But to obviate this, it must be remembered, that the electrified Gun-barrel both attracts and repels light Substances at the same time. Can this Attraction and Repulsion be conceived without the Operation of the electrical Æther both to and from the Gun-barrel at the same time? Does not this point out an Afflux as well as an Efflux? Are not the electrical Repulsions as strong at least as the Attractions? Do not we see light Bodies, either between excited Originally-electrics, or excited Non-electrics, and unexcited Non electrics, dart like a Ball between two Rackets of equal Force? It may be said perhaps,

1. That the suspended Silver may only serve as a Canal of Communication, which discharges the Electricity from the excited Non-electric to the unexcited one; and that when an Originally-electric is placed between the lower Plate in this Experiment and the Floor of the Room, that then the Silver is attracted only, until the lower Plate is saturated with Electricity, and no longer. This is as much as saying that this Effect arises from Electricity, without mentioning in what manner.

2. That this Effect is produced by the electrical Attraction, which gives the Silver a Direction towards the excited Non electric, but that it is kept down near the unexcited one by the Force of Gravity.

vity. Was this the Caufe, the Action of Gravity would operate as much thro' Originally-electrics as through Non-electrics.

60. But I am able to prove the Afflux experimentally, as well as the Efflux, in the following manner. When the Silver lies ftill, though the Motion of the Globes is continued between the two Plates, one fufpended to the Gun-barrel, and the other placed upon an electrical Cake, a Perfon ftanding upon the Floor needs only bring a fmall glafs Siphon in a Veffel of Water, and apply the long Leg thereof near the Plate placed upon the Wax; for upon this the Silver is immediately fufpended; and the Water, which before only dropp'd, now runs in a full Stream, and appears luminous *. Does not, in this Cafe, the Current of the Water point out the Direction of the Current of electrical Æther?

61. When the Machine, *&c.* are placed upon Originally-electrics, if a Man, ftanding likewife upon an Originally-electric, touches the Gun-barrel while the Globes are in Motion, he will receive a Snap or two; after which, though the Motion of the Globe is continued, he will perceive no more Fire from the Gun-barrel. While in this Pofture,

* This Experiment is more elegant, if the upper Plate, attracting the Silver, is fufpended high enough for a Perfon ftanding upon an Originally-electric, conveniently to bring the other Plate under it with one Hand, and to hold a pewter Plate in the other. If the Originally-electric is fufficiently thick, the Silver will not be fufpended; but if the glafs Siphon in a fmall Veffel of Water is brought very near the pewter Plate, the Water runs into the Plate, and the Silver is immediatly fufpended.

Posture, if he touches the Wood-work of the Machine with one Hand, and applies a Finger of his other near the Gun-barrel, at that Instant he receives the electrical Strokes. These continue as long as he touches the Machine, but cease upon his removing his Hand therefrom. Here we see a Circulation of Part of this Man's electrical Fire, which operates in the following manner. First; The Man, by applying one of his Hands to the Machine, becomes a Part thereof; and, by the Motion of the Globes, Part of the electrical Fire, inherent in his Body, is driven upon the Gun-barrel; but it is instantaneously restored to him again, upon his touching the Gun-barrel with his other Hand. Thus he continues communicating the Fire with one Hand, and having it restored to him with the other, as long as he pleases. If, instead of touching the Machine or Gun-barrel, he holds his Finger near either or both of them, you see the Fire go out, and return back, as in a former Experiment.

62. It may be perhaps imagin'd, if one Man touches the Machine, himself and the Machine both being placed upon the Wax, and if another, standing upon the Floor, constantly, or by turns, touches the Gun-barrel, that by these means the Man upon the Originally-electrics might be divested of all his electrical Fire, by constantly continuing the Motion of the Globes, as he receives then no Supply from the Floor. But the contrary proves true; and, after a considerable time, the Strokes from the Gun-barrel are as strong as at first. But here we must observe, that the Gun-barrel suspended will not contain probably at one time a thousandth Part of the whole Quantity of

this

this Man's electrical Fire: Therefore I conceive, that, as ſoon as this Man has parted with any Portion of his neceſſary, his determined Quantity, to the Gun-barrel by the Motion of the Globes, he has it reſtored to him upon any un-excited Non-electric's touching the Gun-barrel, by having the uſual Courſe of the Electricity * inverted.

63. We ſee, from many Experiments, that dry Wood does not conduct Electricity ſo well as that which is wet; and that the Man ſtanding upon the Floor, who rubs the Globes, excites the Electricity ſtronger than the Cuſhions. This I had Reaſon to conceive was owing not to any other Difference, than that of his being more moiſt, and, of Conſequence, more readily conducting the Electricity from the Floor. Therefore I order'd my Machine, and even the Cuſhions to be made damp, by cauſing wet Cloths to be placed upon ſeveral Parts thereof; and found then, that the Electricity was equally ſtrong, as when the Globe was rubbed by the Hand.

64. It remains now, that I endeavour to lay before you a Solution why our Bodies are ſo ſhocked in the Experiments with the electrified Water; the Difficulty thereof I confeſs ſeemed unſurmountable, until I had made the following Diſcoveries.

1. That the Electricity always deſcribed a Circuit between the electrified Water and the Gun-barrel.

2. That the electrical Fire came from the Floor of the Room.

3. That it would not paſs from the Floor quick enough for the Perſon to be ſhook, if his Shoes were dry.

4.

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* For a further Account of this Matter, see *Philoſ. Tranſact.* Vol. XLV. *p.* 101.

4. That the Force was increaſed in proportion to the Points of Contact of Non-electrics with the Glaſs containing the Water.

Then the Solution of this Phænomenon became more eaſy, which I take the Liberty to offer.

1. I have endeavoured to prove by Experiment*, that a Quantity of Electricity is furniſh'd from the neareſt unexcited Non-electrics, equal to that accumulated in excited Originally-electrics and excited Non-electrics.

2. This being ſo, when the Phial of Water held in one Hand of a Man is highly electrified, and he touches the Gun-barrel with a Finger of his other; upon the Exploſion which ariſes herefrom, this Man inſtantaneouſly parts with as much of the Fire from his Body, as was accumulated in the Water and Gun-barrel; and he feels the Effects in both Arms, from the Fire of his Body ruſhing through one Arm to the Gun-barrel, and from the other to the Phial. For the ſame Reaſon, if, in the Experiment with the electrical Mine, a Man places his right Foot upon the lower ſmall Wire, and touches the Gun-barrel with his left Arm, the electrical Force is only [illegible] in that Leg and Arm.

[illegible] then parted with, is inſtan[illegible] Floor of the Room, and that with a Violence equal to the Manner in which he loſt it. To confirm this, [illegible] 54.

[illegible] 4. But

[illegible]

* See more of this in Phil. Trans. Vol. XLV. p. 102.

4. But this Flux of electrical Æther, either from the Floor to the Man, or from the Man to the Water, is prevented for Reasons sufficiently obvious, if the Glass containing the Water be thick; if the Points of non-electric Contact are few; if the Man is placed upon Originally-electrics; or (which is the same thing) if the Soles of his Shoes are dry.

5. As we find that the Electricity passes at least equally quick through dense Mediums, which are Non-electrics, as through those which are more lax and spongy; may we not therefore conclude, that the Cause why we feel most Pain at the Joints of our Arms, and in the Tendons of our Heels †, arises from the Texture in the Tendons and tendinous Ligaments of those Parts?

65. From a due Consideration of the *Phænomena* before us, I take the Liberty of proposing the following Queries:

1. Whether or no the Effects we observe, in Bodies being drawn to and driven from either excited Originally-electrics, or excited Non-electrics, are to be attributed to the Flux of electrical Æther?

2.

† This Pain in the Heels is felt only in the Experiment with the electrical Mine; and it is not perceptible only when you touch the lower small Wire with your Foot, but likewise if you stand upon Non-electrics, which touch this Wire. It has been strongly felt by a Person standing upon a Pedestal of *Portland* Stone near ten Inches in Height, and upon one of Metal more than two Feet. I am of Opinion, that no Mass of Metal, of Dimensions however great, would in the least prevent the Progress of the electrical Power from the Water in the Phials to the Body of the Man.

2. Whether or no, that, which, from its being first discover'd in Amber, we call Electricity, electrical Æther, electrical Power, *&c.* is any other than elementary Fire?

3. Whether or no this Fire does not appear in different Forms, according to its different Modifications? Does it not, when diffused under a large Surface, appear to affect us as Air? When brought towards a Point, does it not become visible, as lambent Flame? When nearer still, does it not explode, and become the Object also of our Feeling as well as of our Hearing? Altho' it does not affect our Skin with the Sensation of Heat; does it not, by its lighting up inflammable Substances, shew itself to be truly Fire?

4. Whether or no this Fire is not connected intimately with all Bodies at all times, though least of all, probably, with pure dry Air? Have we not found and separated it from Water, Flame, even that intense one of Oil of Turpentine, Smoke, red-hot Iron, and from a Mixture thirty Degrees colder than the freezing Point?

5. Have we not proved its Subtility, from its passing through all known Bodies?

6. May we not infer its Elasticity likewise from its Explosions, from its increasing the Motion of Fluids, as well as from its Effect in the Concussion of our Bodies, when we discharge it after we have accumulated it in Water?

7. May not the electrical Machine, from its Uses, be denominated a Fire-Pump, with equal Propriety as the Instrument of *Otto Guerick* and Mr. *Boyle*, that of the Air?

8. Does not the Power we are now Maſters of, of ſeeing the Separation of Fire from Bodies by Motion §, and of ſeeing it reſtored to them again, and even after that Motion has ceaſed, cauſe us rather to incline to the Opinions of *Homberg* (*a*), *Lemery* the younger (*b*), *s'Graveſand* (*c*), and *Boerhaave*,

§ The ſetting in of the Fire to the glaſs Tubes and Globes has always, in theſe Experiments, been viſible both from the Hands and Cuſhions, by which they were rubbed. But as, till now, this Fire was conſidered as coming from the Glaſs, that, obſerved upon the Hands and Cuſhions, was always believed to be ſo much loſt by running down the Inſtruments of Friction into the Floor. I endeavoured to prevent this Loſs, by ſtanding upon Originally-electrics; and found, to my great Surprize, that ſo far from increaſing the electrical Power, by ſtopping what I conjectured was ſo much Loſs, I could excite then no Electricity at all in the Tube and Globes. This Diſappointment, which, I afterwards found, had occurred to Meſſ. *Boſe* and *Allamand*, was the Foundation of my diſcovering the Source of the Electricity, and the Manner of its Ingreſs to the Machine.

(*a*) *Homberg* du ſouphre principe. *Mem. de l'Acad. Royale des Sciences*, 1705. La matiére de la lumiére eſt la plus petite de toutes matiéres ſenſibles — elle paſſe librement au travers et par les pores de tous les corps, que nous connoiſſons. —Que tout l' univers eſt rempli de la matiére de la lumiêre——J'ai mieux donnè à notre ſouphre principe le nom de matiére de la lumiére, que celle du feu, quoique ce ſoit proprement la même choſe.

(*b*) *Lemery* le fils. *Mem. de l'Acad.* 1709. p. 527. La matiére de feu doit être regardée, comme un fluide d'une certaine nature, et qui a des proprietez particulieres, qui le diſtinguent de tout autre fluide. Pag. 8.——Qu'une matiére beaucoup plus ſubtile et plus agitée, qui remplit tous les vuides de l'univers, et ne trouve point les pores ſi étroits, qui ne lui laiſſent un libre paſſage, coule inceſſamment dans les lieux où elle eſt enfermée, et entretient ſon mouvement.

(*c*) *s'Graveſand Philoſoph. Newton inſtitutiones*, *cap.* 1. Ignis in corpora omnia quantumvis denſa et dura penetrat.—— Corporibus

Boerhaave (*d*), who held Fire to be an Original, a distinct Principle, formed by the Creator himself, than to those of our illustrious Countrymen, *Bacon* (*e*), *Boyle* (*f*), and *Newton* (*g*), who conceived it to be mechanically producible from other Bodies?

9. Must we not be very cautious, how we connect the elementary Fire, which we see issue from a Man, with the vital Flame and *Calidum innatum* of the Ancients; when we find, that as much of this Fire is producible from a dead Animal as a living one, if both are equally replete with Fluids?

10. Whether or no it is not highly probable, that, by increasing the Number and Size of the Phials of Water in a certain manner, you might not instantly

ribus sese jungit —— ignem ad certam distantiam a corporibus attrahi —— nulla novimus, quæ ignem non continent —— non ignis æque facile corpora omnia intrat —— corporibus contentus in his a corporibus circumambientibus retinetur. —— Motu celerrimo ignem affici posse.

(*d*) *Boerhaavii Elementa Chem. de igne*, p. 187. et seq. —— Ipse ignis —— semper præsens existit in omni loco —— imo vero in omni corpore, etiam rarissimo, vel solidissimo, æqualiter distributus hæret. —— Haud ergo potui detegere, quod in rerum natura sit vel ullum spatium sine igne.

Ibid, p. 283. Huc usque conabar —tradere ea, quæ verissima addiscere potui de natura illius ignis, quem elementalem appellant philosophi. Illum scilicet, ita considerando, prout creatus ipse in rerum (natura) existet seorsum, extra reliqua omnia creata, quæcunque demum sint, corpora.

(*e*) Vide tractatum *De forma calidi*.

(*f*) *Mechanical Origin of Heat and Cold*, Sect. 2.

(*g*) See Queries at the End of his *Optics*.

inftantly kill even large Animals by the electrical Strokes (*h*)?

66. I cannot conclude thefe Papers, without congratulating that excellent Philofopher and learned Member of this Society the Abbé *Nollet* of *Paris*. This Gentleman, almoft two Years fince, in a Letter to Profeffor *Bofe* (an Extract of which this laft publifhed with a Work (*i*) of his own) without the Knowledge of feveral Experiments fince difcover'd; at leaft none of his Difcoveries have yet fallen into my Hands, did declare his Opinion, (*k*) that the Electricity did not only proceed from the electrified Bodies, but from all others about them to a certain Diftance; (*l*) that the Electricity, as well from Bodies electrified, as from thofe which were not, paffed more readily through denfe Mediums than Air; (*m*) that the Electricity is prefent in all Bodies; that

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(*h*) Monf. *Le Monnier* at *Paris* killed Birds by thefe; and with me, a Linnet and a Rat, much more than half-grown (the largeft I was then able to procure) have been ftruck dead.

(*i*) *Recherches fur la Caufe, et fur la veritable Theorie de l'Electricité.* Wittembergue, 1745.

(*k*) *Voyez Nollet dans les Recherches, &c. du M. Bofe*, Pag. xlv.—La matiére electrique vient non feulement du corps électrifé, mais auffi de tous ceux qui font autour de lui, jufques à une certaine diftance.

Ibid. p. xlix.—Si vous pouvez vous convaincre comme moi, que la matiére qui va au corps électrique vient primitivement de tous le corps environnans, de l'air même, vous aurez bien plus de facilité à expliquer tous les autres effêts.

(*l*) *Ibid.* p. xlvi La matiére electrique, tant celle qui fort du corps électrifé, que celle qui vient des environs à ce même corps, fe meut plus facilement dans les corps denfe que dans l'air même.

(*n*) *Ibid.* p. xlvii.

(*n*) that this Matter always tends to an *Æquilibrium*, and endeavours to occupy those Spaces in Bodies, which have not their necessary Quantity: All which Assertions may now be proved by Experiments.

67. You see, Gentlemen, by my asserting, that what we have hitherto called electrical *Effluvia*, do not proceed from the Glass, or other Electrics *per se*, I differ from *Cabeus*, *Digby*, *Gassendus*, *Brown*, *Des Cartes*, and very great Names of the last as well as the present Age. My differing from them would be Presumption indeed, were I not induced thereto, by Observations drawn from a Series of Experiments carefully conducted, to which many of you have been Witnesses, and to whom I may therefore appeal, for taking what may seem so extraordinary a Step. I have constantly had in View that excellent Maxim of Sir *Isaac Newton* laid down in his *Optics*, that, "as in Mathematics, so in Natural Philosophy, the " Investigation of difficult Things by the Method of " Analysis ought ever to precede the Method of Com- " position. This Analysis consists in making Experi- " ments and Observations, and in drawing general " Conclusions from them by Induction, and admitting " of no Objections against the Conclusions, but such " as are taken from Experiments, or other certain " Truths. For Hypotheses are not to be regarded " in Experimental Philosophy. And although the " arguing from Experiments and Observations by " Induction

(*n*) *La meme.* Cette matiére tend à l'équilibre, et s'empresse de remplir les espaces, qui se trouvent vuides des parties de son espece.

"Induction be no Demonſtration of general Con-
"cluſions; yet it is the beſt Way of arguing which
"the Nature of Things admits of, and may be
"look'd upon as ſo much the ſtronger, by how
"much the Induction is more general. —— By this
"Way of Analyſis we may proceed from Com-
"pounds to Ingredients, and from Motions to the
"Forces producing them; and, in general, from
"Effects to their Cauſes, and from particular Cauſes
"to more general ones, till the Argument ends in
"the moſt general." I am deſirous, that what is contain'd in theſe Papers, you will be pleaſed to regard rather as the rude Outlines of a Syſtem, than as a Syſtem itſelf; which, I am in Hopes, Men of better Heads and more Leiſure will proſecute: And if hereafter, from being poſſeſſed of more Obſervations than we at preſent are Maſters of, any Opinions in theſe Papers ſhall be found erroneous, I at all times ſhall be willing readily to retract them. I rely upon your wonted Candour, and am,

With the greateſt Truth,

Oct. 20. 1746. *Gentlemen,*

Your moſt devoted and moſt humble Servant,

W. Watſon.

FINIS.

The CROONEAN LECTURES.

E R R A T A.

N°. 459. Art. XV. Mercurius à Venere ſublatus *Maii* 17, 1737. p. 630, 1, 2. this intire Article is to be expunged; being inſerted more accurately in N°. 450. p. 394.

N°. 474, p. 167, l. 13. *for* Manteufet, *read* Manteuffel.

E R R A T A in Vol. XLIV.

N°. 479, p. 101, l. 2. *for* ſoun- *read* ſo un-

N°. 481, p. 299, l. 9, *read* Grains $37\frac{4}{10}$ above 2 Oz.

N°. 482, Sheet (Zz) the Pages are double and confus'd by a Miſtake of the Printer; and therefore after the Numbers of the Pages in this Sheet I have in the *Index* ſet (Zz).

Ib. p. 408, l. 24, *for* Mr. Juan Antonio de Loa, *read* Don Antonio de Ulloa.

Ib. p. 433, l. ult. *for* the other Side Chalk, *read* the other Side of a Subſtance like Chalk, but much harder.

N°. 483 in the Title *read* for the Months of *March*, *April*, *May*, *June*, and *July*, 1747. *Ib.* p. 456, l. 20, *read* Tab. II. *Fig.* 8.

N°. 484, p. 630, l. 30. *read* Tab. I. Fig. 1.

Ib. p. 680. to the Mark † *add*, Sir *Iſaac Newton*'s Thermometer was made of *Line-ſeed* Oil. See his Scale of Heat, *Phil. Tranſ.* n. 270. p. 824.

Ib. p. 700, in the Note *dele* (p. 111.)

To the Binder.

Next after this follow the *Crooneán Lectures* for the Year 1747. and after them place the *Index* to Vol. XLIV.

AN

INDEX

TO THE

Forty-fourth VOLUME

OF THE

Philosophical Transactions,

For the YEARS 1746, and 1747.

N.B. There being in this Volume 2 *Supplements*, one the *Croonean Lectures* for 1746, the other for 1747, *a* denotes the first, *b* the latter; thus *pr. a.* or *pr. b.* denote the Preface of each Year's Lectures, *s. a.* or *s. b.* stand before the Number of the Pages of the Lectures.

The Pages 351 — 358 are printed twice over; therefore to the second Numbers is added Zz. *e. gr.* 351, Zz.

A

America

Ferguson

H.

Hardouin's

King

Miles

N.

R.

R.

S.

Water-

INDEX.

FINIS.

To the BOOKBINDER.

Place the *Dedication* to the *Emperor* immediately after the Title-Page of *Part* I. before N°. 478.
Place the *Title-Page* of *Part* II. before N°. 482.
Place this *Index* after the *Croonean Lectures* for 1747. by *Browne Langrish* M. D.

THE CROONEAN LECTURES ON MUSCULAR MOTION,

By BROWNE LANGRISH M. D. *and* Fellow *of the* ROYAL SOCIETY.

Read before the

ROYAL SOCIETY

In the Year MDCCXLVII.

Being a SUPPLEMENT to the *Philosophical Transactions* for that Year.

There can be no greater Presumption in Favour of a Scheme, than that it is simple, and of a Piece with the known System of the Universe.

MEAD's *Introd. to his Essays on Poisons*, Edit. 3.

LONDON:

Printed for C. DAVIS, over-against *Gray's Inn Gate* in *Holbourn*, PRINTER to the ROYAL SOCIETY, M.DCC.XLVIII.

To the REVEREND

STEPHEN HALES, D.D.

Fellow of the ROYAL SOCIETY,

These THREE LECTURES

ON

MUSCULAR MOTION,

Are humbly dedicated, as a ſmall Acknowledgment for the many and great Advantages received from his moſt curious, learned, and indefatigable Reſearches into Nature; and for the many perſonal and particular Favours which he has been pleaſed to confer on

His Moſt Obedient, and

Moſt Humble Servant,

BROWNE LANGRISH.

PREFACE.

IN the Year 1733 *I published an Essay on Muscular Motion, founded on the* Newtonian *Philosophy of* Attraction *and* Repulsion ; *and though the Thoughts were hastily put together, and the Performance in general, very inaccurate, yet I must own I have not met with any Reason, since that Time, to alter my Sentiments in regard to the Cause and Manner of Contraction in the muscular Fibres.*

I did not think indeed of ingaging myself any further in solving such abstruse Phænomena ; *but being desired by my ever honoured Friend Sir* Hans Sloane *Baronet, to make some Experiments which might illustrate and confirm the Truth of my* Hypothesis, *I readily and chearfully complied with his Commands ; and afterwards I drew up the following Lectures, which were by Order of the* President *and* Council *read before the* Royal Society, *as the* Croonean Lectures *for the Year* 1747.

pursuant

pursuant to the Will of the late Lady Sadleir *.

The Foundation of my Scheme is laid upon those Hints which Sir Isaac Newton *has given us in the* Queries *at the End of his incomparable Book of* Opticks; *together with what Dr.* Pemberton *has said in his very learned Introduction to* Cowper *on the Muscles. How well it is executed is most humbly submitted to my* Superiors. *I do not know that I have advanced any thing inconsistent with true Philosophy; or to any of the known Laws of the animal Oeconomy. I have the Satisfaction to see, in the second Volume of the late Dr.* Desaguliers's *experimental Philosophy* §, *that he approved of what I had formerly published, beyond all other Accounts of Muscular Motion; and I must confess, that meeting with Countenance from so good a Judge gave me great Encouragement to pursue the Enquiry.*

* *Whose first Husband was Dr.* Croone, *and at his Request these Lectures were founded.*

§ *Page* 393.

THE

THE CROONEAN LECTURES ON MUSCULAR MOTION.

LECTURE I.

SECT. I.

Read at a Meeting of the Royal Society, *on* March 5, 1746-7.

THE external Superficies of every Muſcle, its Origin and Inſertion, the Uſe of its Action in regard to the Animal Oeconomy, from what *Plexus* the Nerve comes, whence the Artery which ſupplies it ariſes, and to what Vein the Blood is carried, are Diſquiſitions very worthy the Knowledge of every *Phyſician* and *Surgeon*; in order to diſcover the true Seats and Cauſes of many Complaints, and to point out the moſt rational Methods of Cure.

II.

But as theſe Reſearches have nothing to do with the immediate Cauſe of Muſcular Motion (the Ex-

plication of which seems to be the chief Design of the Learned *Founder* of these Lectures) I shall wave all such Enquiries, and proceed directly to investigate by what Means a Muscle so instantly and forcibly contracts itself, or shortens its Length, at the Command of the Will; and this I shall endeavour to do by such Rules and Laws as are simple, uniform, and intirely agreeable to the known System of the Universe.

III.

In order to the easier Illustration of this wonderful and important Property in the Fibres, I shall give you, 1*st*, An analytical View of the component Parts of a Muscle. 2*dly*, I shall shew the true Cause of Cohesion, Tension, and Elasticity in the animal Fibres. 3*dly*, I shall make it appear, that every Fibre constituting a Muscle, is, in its ultimate Division, tubular, and not a Chain of Vesicles or Bladders. And, *lastly*, I shall prove it to be more than probable, that Muscular Motion proceeds from the *attractive* Quality of the constituent Particles of every Fibre being increased and strengthened by the Addition of some *æthereal Matter* flying out from the Extremities of the Nerves; and that this is instantly occasion'd by the Will, and ceases again as soon.

IV.

By a chemical *Analysis* of a Muscle dissected from the Buttock of a lean Ox, which weigh'd exactly two Pounds *Averdupois*, I procured,

1.

	Oz.	Dr.	Gr.
1. Lymph — —	xxiiii	xiiii	xv
2. Volatile Salt — —	i	iiii	xxv
3. Oil — —	ii	vi	xx
4. *Caput mortuum* —	ii	ix	v
5. Loſt in Diſtillation, which I preſume was moſtly mere Air	—	xii	xvi
	32	00	00

V.

There being no *Averdupois* Weights in the Shops, leſs than Quarters of Ounces, I order'd ſome to be made of a Drachm, and others of two Drachms. The Drachm weigh'd 27 Grains; ſo that, by caſting up the Grains into Drachms, and the Drachms into Ounces, we have the exact Weight of each of the above conſtituent Principles according to the Weight they were firſt of all weigh'd with.

VI.

It may be proper alſo to obſerve, that the *Apparatus* I made uſe of in this Proceſs, was the ſame which I communicated a Deſcription of to the *Society* ſome time ago, and which is ſince publiſhed in the *Philoſophical Tranſactions* N°. 475; except that the Retort I now uſed was made of Copper, in order that I might remove it from the the Sand-Heat into the actual Fire, without unluting any Part of

the *Apparatus*, when no more Matter would ariſe by means of the Sand-Heat.

VII.

By this Method I could increaſe the Fire till the Bottom of the Retort was red-hot, without any Danger of breaking my Recipients; a Contrivance which may be uſeful in many chemical Proceſſes.

VIII.

From the above-mention'd Experiment we have evident Proof of the Proportions and Qualities of the ſeveral Principles, or conſtitutive Parts of the muſcular Fibres; and let no one be ſurpriſed that the watery or phlegmatic Principle abounds ſo much as to be nearly $\frac{13}{16}$ Parts of the whole Maſs, ſince we know that dried Bones, and many other Things as unpromiſing, afford half their Weight of Water.

IX.

That the Particles of Water are endued with a ſtrongly attracting Power, and are highly ſerviceable as a Band of Union in the Formation and Growth of every thing, animate and inanimate, is not only manifeſt from the great Quantity employed in the Growth of Animals and Vegetables, but alſo in our own manual Operations, ſuch as making ſeveral Sorts of Glue, Paſtes, Bricks, and ſuch like, where the watery Particles prove a very durable and powerful *Copula*, and are not to be all of them ſeparated again,

again, even by a very intenſe Fire: Water is to be regenerated from Bricks and Tiles after they have been burnt in the Kiln.

X.

The drieſt Wood, Part of a Mahogony-Table, which had ſtood by the Fire many Years, being raſped and put over the Fire in a Copper Retort, afforded a conſiderable Quantity of Water.

XI.

In ſhort, take away the Water from the moſt ſolid animal and vegetable Bodies, and they immediately become mere Duſt.

XII.

I don't apprehend that this conſiderable Quantity of Water, which is to be procured from ſuch ſolid Subſtances as Bricks, Wood, or even from the muſcular Fibres, remains in diſtinct Drops or Spherules, whilſt it is a Part of ſuch ſolid Subſtances; but I conceive that the *Minima*, or primary ſolid Particles of Water may be attracted by, and actually joined with, the earthy, ſaline, and other component Particles, ſo as to compoſe the ſeveral Degrees of Hardneſs, according to their reſpective Proportions and Qualities; and when theſe ſeveral conſtituent Principles are diſunited again, by the Power of Fire, or by the Length of Time, they riſe up into the Air, or into the

the Recipient, according to their Divisibility and Levity; first Water, next Salt, then Oil.

XIII.

Nature seems to delight in Transmutations. Many Kinds of Fluids are easily converted into dense Bodies. We all know how soon Water is capable of being turn'd into a very solid friable Stone, by the Power of Cold. Mercury also is easily turn'd into a hard brittle Metal; and both these return to their former State of Fluidity by means of Heat. And a Solution of Copper in Spirit of Nitre being pour'd on Oil of Tartar, both Liquids instantly become Verdigrease in a dry Powder.

XIV.

From what has been said we may observe, that Water, or the watery Particles not only make up much the greatest Part of the muscular Fibres, but, by mutually attracting, and being attracted by the other component Particles, they greatly contribute towards their Cohesion and Elasticity; for a fluid Particle will be fixed, and become a Part of a Solid, as soon as there is an attractive Force sufficient to effect its Cohesion with the other solid Parts, though it returns to its former State of Fluidity upon the Analysis of the compounded Body.

XV.

I would not by this be understood as if I design'd to exclude the other Principles from their Share which

which they give to the true Degree of Firmneſs and Elaſticity in the Fibres; the ſaline, ſulphureous, and earthy Parts are all enduced with a ſtrongly attracting Power; and when brought into Contact with each other, or with the watry and aereal Particles, they give Firmneſs and Solidity to the Compoſition.

XVI.

Water ſeems to be the *Gluten* by which the other Principles are wrought up. Too much Water in the Compoſition renders the Fibres ſoft and lax; as in Children, and anaſarcous People. Too little Moiſture occaſions a ſtiff, rigid Fibre; as in old Age. There is a certain Degree of Texture and Coheſion neceſſary towards muſcular Motion in its greateſt Strength.

XVII.

I have ſhewn in a former Treatiſe *, that Air is very inſtrumental in fixing and uniting the other Principles which conſtitute an animal Fibre; for in the moſt ſolid Parts of the Body, where the Coheſion is ſtrongeſt, we find great Plenty of Air. That the Air-Particles are capable of being united, and fix'd to ſolid Bodies, and by that means may be eſteemed a Part of their Compoſition, we have many evident Proofs in Dr. *Hales*'s *Analyſis of the Air*; and that thoſe Particles do in their fixed State ſtrongly

* Modern Theory of Phyſic, *p.* 56.

ſtrongly attract the other component Particles, is evident, it being well known, that the moſt ſtrongly repelling and elaſtic Particles, when in a ſeparate State, are the moſt ſtrongly attracting, when fixed to other Bodies.

XVIII.

Now, according to Dr. *Hales*, ſince a much greater Proportion of Air is found in the ſolid than in the fluid Parts of Bodies; may we not with good Reaſon conclude, that it is very inſtrumental, as a Band of Union in thoſe Bodies; thoſe Particles (as Sir *Iſaac Newton* obſerves*) receding from one another with the greateſt repulſive Force, and being moſt difficultly brought together, which upon Contact cohere moſt ſtrongly? And if the Attraction or Coheſion of an unelaſtic Air-Particle be proportionable to its repulſive Force in an elaſtic State, then ſince its elaſtic Force is found to be vaſtly great, ſo muſt that of its Coheſion alſo. Let us add to this, that the Air generated from the fleſhy Fibres, in the Experiment above-mention'd, was not ſeparated without great Violence; for it did not riſe in any Quantity, till the Clouds did, which contain'd and brought over the Salt and Oil: Whence it is evident that the aëreal Particles are firmly fix'd, and conſequently are very inſtrumental in the Union of the other conſtituent Principles.

XIX.

* Opt. *Qu.* 31.

XIX.

Sir *Isaac Newton* * thinks, that not only Water and Air are convertible into dense Bodies, but that even Light may become a Part of gross Bodies, and that they may receive much of their Activity from the Particles of Light which enter their Composition. It is the Opinion also of Monf. *Homberg*, that Light or Fire is a Part of the Composition of all Things; though in the Analysis of Bodies it is always lost, escaping the Skill of the Artist, and passing through the closest Vessels.

XX.

We all know that solar Fire, or Light, adds Weight to Lead, Tin, or Regulus of Antimony, when exposed to the Fire of a burning Glass, though they otherwise lose much in Smoak and Steam. But to proceed:

XXI.

These then being then the component Parts of the muscular Fibres, our next Task is to shew the Cause of their Tension and Elasticity.

XXII.

That all the muscular Fibres of the Body are in a State of Tension, during Health, is manifest from every

* Opt. *Qu.* 30.

every Incision made across them, when the two Segments of the Muscle so divided, retire, one to its Insertion, and the other to its Origination; that is, every Fibre is always stretched out beyond its natural State of Rest or Quiescence, so that both Ends of it retract a considerable Distance after being cut asunder. Now there are two Things which seem to be principally concerned in this Affair; *viz.* the Impulse and Pressure of the circulating Fluids, always distracting the Fibres, and a constant *Nisus* or Endeavour in the constituent Particles of the Fibres to run closer together, when so distended, by means of their mutual Attraction towards each other.

XXIII.

The Equilibration which is ever preserved between the antagonist Muscles, in a healthy State, unless when the Will directs it otherwise, arises from this *Vis Restitutionis*; which being stronger or weaker according to the Degrees of Tension, and the Degrees of Tension depending upon the Velocity and Quantity of Fluids circulating through every Fibre; it follows, that as long as the Fluids have the same free Access to every voluntary Muscle, so long will the *Æquilibrium* be maintained.

XXIV.

In an Essay which I publish'd on this Subject of muscular Motion, in the Year 1733, I endeavoured to prove that every the least Corpuscle of Matter is endued with an attractive Virtue on one of its Sides,

and

and a repulsive Power on the other, something similar to the Loadstone; and this I was first of all induced to believe, from what Sir *Isaac Newton* observes in his Opticks, *Qu.* 31.

XXV.

When any saline Liquor (*says he*) is evaporated to a Cuticle, and let cool, the Salt concretes in regular Figures; which argues, that the Particles of the Salt, before they concreted, floated in the Liquor, at equal Distances, in Rank and File; and by Consequence, that they acted upon one another by some Power, which at equal Distances is equal, at unequal Distances is unequal: For, by such a Power, they will range themselves uniformly, and without it they will float irregularly, and come together as irregularly. And since the Particles of *Iceland* Chrystal act all the same Way upon the Rays of Light, for causing the unusual Refraction, may it not be supposed, that in the Formation of this Chrystal, the Particles not only ranged themselves in Rank and File for concreting in regular Figures, but also, by some kind of polar Virtue, turned their homogeneal Sides the same Way?

XXVI.

And again, we are taught by the same *Great Man*, that Fire is the most simple of all known Bodies, and consequently the most immutable; that each Ray of Fire or Light has Sides differently affected, and which have different Properties; and that *Iceland*

 Chrystal

Chryſtal is found to attract a Corpuſcle of Fire, if one of its Sides be turned towards the Chryſtal, and repel it, if the other be; for one and the ſame Ray is here refracted ſometimes after the uſual, and ſometimes after the unuſual manner, according to the Poſition which its Sides have to the Chryſtal; and ſince the Chryſtal, by this Diſpoſition or Virtue, does not act upon the Rays, unleſs when one of their Sides of unuſual Refraction looks towards that Coaſt, this argues a Virtue or Diſpoſition in thoſe Sides of the Rays, which anſwers to, and ſympathizes with, that Virtue or Diſpoſition of the Chryſtal, as the Poles of two Magnets anſwer to one another.

XXVII.

We are fully perſuaded, that, in the Chryſtallization of Salts, they could not ſo regularly and conſtantly preſerve their peculiar Shapes, Forms, and Figures, if every Particle of them had not its determinate Poles: For granting that the component Particles of each kind of Salt have a peculiar Shape and Size, different from any other kind of Salt, yet if they had a Power of uniting with each other indifferently, at their Tops, Sides, and Bottoms, one would think they could not always coaleſce into Chryſtals of the ſame regular Figure: But if the conſtituent Particles of every kind of Salt have their determinate Poles, then they cannot poſſibly unite with each other, but when their Poles ſquare with one another, and conſequently they will always fly together, and be joined at ſuch Points, only where their correſponding Poles are; which muſt of courſe conſtantly produce the ſame regular Form and Figure

in

in every Aggregate of ſuch particular ſaline Particles.

XXVIII.

Hence it is, that *Salt Armoniac* ſo elegantly imitates the Branches of a Tree; *Salt of Hartſhorn* a Quiver of Arrows; *Salt of Tin* ſhoots into Lines like little Needles, which ſpread themſelves every Way from a Point, as from a Centre, ſo as to repreſent a Star, *&c.* Now can it be imagined that theſe, or any other kind of Salts, would immutably and perpetually coaleſce into Chryſtals of the ſame regular Figure and Shape from any other Principle?

XXIX.

Since therefore we have ſo much Reaſon to believe that Salts of all kinds, and even the Rays of Light are endued with a polar Virtue, that is, every Corpuſcle attracts on one of its Sides, and repels on the other; and ſince it is a well known Axiom, that *Nature is ever frugal in Principles*, I think it not at all unphiloſophical, or contrary to any of the known Laws of Nature, to believe that every Particle of Matter in the World is endued with an attractive and repulſive Property.

XXX.

Thus then, if the conſtituent Corpuſcles of the muſcular Fibres are formed together according to this Law, if they are all united at particular Points cor-

corresponding to their attractive Virtue, it follows, that wherever a muscular Fibre is stretched out to the least Degree of Tension, some of its Particles will touch each other in fewer Points; whilst others may possibly be disunited and removed from each other, though perhaps to inconceivable small Distances: Hence there will be a constant *Nisus* in the separated Particles to get together again; and this *Vis Restitutionis* will be stronger or weaker, according to the Number of Corpuscles so disjoined, and their attractive Virtue.

XXXI.

If the Power of the circulating Fluids (and I think it cannot be denied) be sufficient, from the first Beginning of the Circulation of the *Fœtus*, and so on as long as Life continues, to distend the Fibres beyond the Size they would otherwise be of, by reason of their corpuscular Attraction; this distractile Power must always be the Occasion of some Degree of Tension in them: And if, upon the Removal of this Tension, the component Particles have a Property of running closer together, and contracting the Fibres in their Length, by the means above-mention'd, this must be the true Cause of Elasticity in the Fibres.

XXXII.

* Hence therefore it follows, that since the Fibres are always in a State of Distraction, by the Quantity and *Momentum* of the circulating Fluids, and as they are ever endeavouring to shorten themselves, by means of their corpuscular Attraction, their Elasticity must

muſt depend upon Tenſion; for the Fibres could have no Power to retract, or abbreviate their Length unleſs they were extended beforehand by ſome certain Force.

XXXIII.

It is not a ſufficient Objection againſt this Scheme to ſay, that if we depend upon what is viſible, we ſhall never ſee the dry ſolid Fibres, or Particles of any ſolid Body, once divided or drawn out of Contact, coaleſce or unite again, or recover the cloſe Contacts they had before; without ſome fluid Medium ſuperadded.

XXXIV.

'Tis true, when a viſible Crack or Flaw happens in any dry, hard, ſolid Body, ſuch as a Steel-Spring, or a dry, rigid, wooden Bow, the Rupture will always continue, by reaſon the ſever'd Particles cannot be brought again into the Sphere of each other's Attraction without the Intervention of ſome Medium; but then it does not follow from hence, that ſuch a Spring or Bow cannot be bent at all without breaking; or that the conſtituent Particles, which muſt neceſſarily be diſplaced by ſuch a diſtending Power, do not fly together again by their attractive Virtue, when removed only to ſuch minute Diſtances.

XXXV.

The *Minima*, or primary Atoms of all Bodies are non-elaſtic, as being perfectly hard, ſolid, and inſeparable; and therefore Elaſticity muſt proceed from the Aggregate, or Compoſition of ſuch Atoms, which, by

by being capable of changing their Situations, according to the impressed Force, and being endued with a powerful attracting Virtue, they instantly resume their former Positions, when left to themselves to obey those Laws which the *Great Creator* hath impress'd upon them. As for Instance: Whilst a common Steel Spring, or any such elastic Body, is not extended or bent, we presume every individual Particle of it to be at Rest; that is, they are all situated, in regard to each other, according to their Poles, and embrace one another by their common Principle of Attraction; but no sooner is such a Spring bent, by some impressed Force, but many of its Particles on the convex Side, must of course touch in fewer Points, or perhaps be disjoin'd from each other, though to the most minute Distances that can possibly be; whilst other Particles, on the concave Side of the Spring, must necessarily slip upon, or be crouded over one another. Hence it will follow, that if those Particles which are separated from each other, or touch one another in fewer Points than usual, are yet so near each other as to be within their Sphere of Attraction, and not at all, or very little alter'd in regard to their Poles, they will consequently attract each other very strongly, and fly together again, as soon as the impressed Force is removed; whereas it is no unreasonable Conjecture to suppose, that those Particles on the concave Side of the Spring, which are compressed, and as it were rumpled over one another, may be so much alter'd from their former Positions, that their Poles do not now answer to each other; and if not, they will repel one another, according to their respective

Power

Powers, till they have attain'd their former Situations, or, in other Words, till the Spring has recover'd its former Shape.

XXXVI.

The ſame Principles of Attraction and Repulſion are the Cauſe of Reſtitution or Elaſticity in all other kind of Bodies. When a muſcular Fibre is ſtretched out longer than uſual, it is moſt certain that ſome of its component Particles muſt ſlip upon or by one another, or elſe be removed at exceeding ſmall Diſtances from each other; ſo that if the impreſſed Force be too violent, if the Tenſion be carried ſo far as to diſjoin a great Number of the component Particles beyond their Sphere of Attraction, the Fibre will continue to grow weaker and weaker till it breaks: But it is as evident, on the other Side, that when a ſtretched-out Fibre does not break, but retracts itſelf into its former Shape and Dimenſions, upon the Removal of the extending Power, the Particles which were diſplaced return again to their proper Poſitions, merely by the means of their attractive Virtue.

XXXVII.

Now all this being ſo agreeable to thoſe *Laws of Nature* which that divine Man Sir *Iſaac Newton* has diſcover'd to us, I think we have good Reaſon to conclude it to be the true Cauſe of Elaſticity in the animal Fibres.

XXXVIII.

The Elaſticity in the Air indeed, or in Water agitated by Fire, or in all the Exhalations proceeding from the Earth, ariſes from the Principle of Repulſion only; for the Particles of Vapours, Exhalations, and Air, ſtand at a Diſtance from one another, and endeavour to recede as far from one another as the Preſſure of the incumbent Atmoſphere will admit them. No Power yet known is able to compreſs the Air-Particles within the Sphere of their Attraction towards each other, ſo as to deſtroy their elaſtic Property; and yet ſingle primary Particles of Air are continually attracted by other Bodies, and conſolidated with them, till by the Action of Fire, or Fermentation, they are ſeparated again, and reſtored to their repulſive State.

XXXIX.

Hence we may obſerve, that Elaſticity, in different kinds of Things, or in Matter differently modefied, may ariſe from two ſeveral Cauſes, *viz. Attraction* and *Repulſion*; and perhaps, in many Inſtances, from the Influence of both at one and the ſame time.

XL.

Whenever any kind of Matter is actuated by Fire, by Fermentation, or diſſolved by any Menſtruum, ſo as to throw off its Particles in ſubtile Vapour, there will

will be a conſtant Endeavour in thoſe Particles to recede further from each other; ſo that the more they are confined, or compreſſed, the greater will be their claſtic Power: Whereas in ſolid Bodies, this Property of Elaſticity proceeds chiefly from Attraction, or a *Niſus* in the component Particles to fly back, or run into cloſe Contacts again, whenever they happen to be ſtretched out, or bent, ſo as to touch each other in fewer Points.

XLI.

From what has been ſaid we may deduce the following *Corollary*, *viz.* That whenever *Elaſticity* proceeds from the Principle of *Repulſion*, as it does in Air, Vapours, *&c.* ſome *Compreſſion* is neceſſary, in order to force the claſtic Matter into a narrower Compaſs than it would otherwiſe poſſeſs; but when it ariſes from *Attraction*, as in the muſcular Fibres, and all ſolid Bodies, ſome *diſtractile Force* is requiſite to disjoin the component Particles from their uſual Contacts, before it can exert its Power; and perhaps, for want of attending to this Difference, ſo many various Opinions may have ariſen concerning the Cauſe of Elaſticity.

XLII.

In my next Lecture I ſhall conſider the Shape of the muſcular Fibres, and the Cauſe of muſcular Action.

LECTURE II.

XLIII.

Read March 26. 1747.

IN my former LECTURE I endeavour'd to explore the ſeveral conſtituent Principles of the muſcular Fibres, and to ſhew the true Cauſe of their Coheſion, Tenſion, and Elaſticity. In this, I hope, I ſhall make it appear, 1*ſt*, That every Fibre conſtituting a Muſcle is *tubular*, and of a *cylindrical* Shape, or very nearly ſuch; and not a String, or Chain of Bladders, according to an *Hypotheſis* which has been too long and too generally received. 2*dly*, That the corpuſcular Attraction between the component Particles of the Fibres is ſo far increaſed and ſtrengthen'd by the Influence of the nervous *Æther*, which is always at the Command of the Will, as to purſe up and ſhorten every Fibre in its Length, whereby an Intumeſcence ariſes in the Belly of the Muſcle, though it is contracted in its other Dimenſions, ſo as, in the Whole, to poſſeſs leſs Room.

XLIV.

Being favoured with the Uſe of a moſt excellent Microſcope, I made the following Experiments.

1*ſt*,

XLV.

1*st*, I divided ſome Fibres as minutely as I poſſibly could, from the Heart of an Ox, from a Part of the Diaphragm, from the intercoſtal Muſcles, and from a Rump of Beef; all which were boiled to ſuch a Degree of Tenderneſs, that we ſeparated ſome Fibres with the Point of a Needle, which were not viſible till placed under the Microſcope, and even then they did not appear bigger than Hairs, though others, which looked like Hairs to the naked Eye, were magnified to the Size of Wheat-Straws. All theſe ſeem'd to be Faſcicles of continued Tubes, as far as we could view them, without any Partitions or Cells.

XLVI.

2*dly*, Upon rending a Muſcle, which was taken from a Knuckle of Veal, and boil'd for four or five Hours, ſeveral of the tranſverſe, as well as longitudinal Fibres appear'd very diſtinctly; which being placed under the Microſcope, and having a ſtrong focal Light caſt upon them by means of a *Florence* Flask fill'd with Water, they ſeem'd to be ſhrunk up, either by being boil'd ſo long, or by being expoſed to the Air, ſo that their Surfaces ſeemed to be unequal and corrugated; which is what Mr. *Leeuwenhoek* * ſays deceived him at firſt, ſo as to make him

* *Anatom. & Contempl.* p. 43.

him think these Corrugations were so many Vesicles or Cells; but he soon discover'd his Mistake. In some of the Fibres I could plainly discern a dark List running in the Centre, from one End to the other; but what it was, I could not discover.

XLVII.

3*dly*. Having observed the muscular Fibres in the Leg of a Sea-Crab to divide very easily and distinctly from one End of the Muscle to the other; we placed a great many of them under the Microscope, but could not discern any thing like Partitions or Cells.

XLVIII.

In short, Mr. *Leeuwenhoek* * assures us, that the minutest Fibres that are visible to the naked Eye appear through a good Microscope to be invested with a Membrane, which includes within it not one simple Body, but a Bundle of still finer Fibres, the last, or smallest Order of which he thinks to be simple hollow Tubes.

XLIX.

This perhaps is as good Authority as we can have from the Assistance of Microscopes; but if we may be allowed to deduce our Arguments from the Analogy which the muscular Fibres bear to some other Parts

* *Phil. Trans.* N°. 367.

Parts of the Body, whose Shapes we are well acquainted with, the Reasonableness of this Opinion may appear yet stronger.

All Anatomists agree, that the muscular Fibres have their Rise from the Extremities of the Nerves and Blood-Vessels; every Fibre being supplied by a Branch of a Nerve, and an Artery, and having also a Vein arising from it.

LI.

That the nervous *Capillamenta* are Cylinders is not denied by any one that I know of; and though the Arteries have been for a great while thought to be conical, yet the ingenious Dr. *John Stephenson* * Fellow of the *Royal College* of *Physicians* at *Edinburgh* hath evidently demonstrated the whole arterial System to be Cylinders, frequently divided and subdivided, still terminating in Numbers of small Cylinders, the Aggregate of which is always of greater Capacity than the Trunk or larger Cylinder before the Ramification.

LII.

May we not therefore very reasonably believe, from the Simplicity and Uniformity in all the Operations of Nature, that the muscular Fibres partake of

* Medical Essays, *Vol.* V.

of the ſame Figure with thoſe from whence they have their Riſe; eſpecially when ſuch a Shape (as will appear in the Sequel) is more proper for all the Functions of a Muſcle than any other whatſoever?

LIII.

I don't mean by this, that every Fibre of every Muſcle is a perfect and regular Cylinder from one End to the other; many of them may be thicker in their Coats, and larger in their Bores about the Middle than towards each End, ſimilar to the Shape of the Muſcle; but what I think the moſt reaſonable Opinion is, that the ſmalleſt *Fibrillæ* are hollow Tubes not divided into an infinite Number of Cells or Veſicles.

LIV.

The longitudinal, red, fleſhy Fibres ſeem indeed to be contorted and bound about in many Places, with white, ſpiral, and tranſverſe Ramifications of the Nerves; but I can ſee no Reaſon to believe that theſe nervous Filaments divide the longitudinal fleſhy Fibres into ſeveral Apartments or Cells; I rather think that they only dip into the Cavities of the Fibres, in order to convey into them the *æthereal Medium*, which is contained in the Nerves.

LV.

LV.

Before the Laws of Nature, and the Animal Oeconomy were ſo well known as they are now, I don't wonder that the veſicular Scheme was thought a reaſonable one, till it came to be examined by ſtrict Rules and Experiments. The common Experiment of raiſing Weights by blowing up Bladders might ſeem, at firſt Sight, a very feaſible Way of explaining muſcular Motion; and without Doubt this firſt of all gave Birth to the veſicular *Hypotheſis*.

LVI.

But the Fallacy of this Experiment was not diſcover'd for want of attending to the Difference between Bladders which have been already blown up, and dried, and ſuch as are recent and ſupple.

LVII.

If a String of dry Bladders, which have been once diſtended as far as they could bear without burſting, and are now again ſqueezed cloſe, and ſtretched out only in their Length, by means of a Weight hung at their Bottom; I ſay, if ſuch a String of Bladders be blown up, it will undoubtedly diſtend their tranſverſe Diameters ſo as to raiſe up the Weight: But in all tender yielding Veſicles, ſuch as the muſcular Fibres moſt certainly are, in their laſt, or ſmalleſt Order, it is well known, that if they were to be inflated with Air, or any ſuch-like Matter, it would

them in every Direction alike; they would grow longer as well as wider. Hence it follows, that if the abovemention'd Experiment was to be made with Bladders just as they are taken out of animal Bodies, it would not answer the Purpose, as is evident from blowing up those of *Calves*, *Hogs*, &c.

LVIII.

The muscular Fibres, it is true, are always in a State of Tension, but then this Tension is very far from being to their utmost Stretch; so that, were they to be inflated in the manner above-mention'd, every Muscle would necessarily increase in Length as well as Breadth.

LIX.

Another insuperable Difficulty belonging to the vesicular *Hypothesis*, is how to blow up a Bladder open at both Ends; which every Vesicle is supposed to be, by having a free Communication with the Blood-vessels.

LX.

Having therefore so much Reason to conclude, that the muscular Fibres, in their ultimate Divisions, are not cellular, but tubular, let us proceed to shew the Manner and Cause of their Contraction.

LXI.

A Muſcle in its Motion very evidently grows leſs in Bulk.

This Propoſition is clearly demonſtrated by that famous Experiment communicated to the *Royal Society*, by Dr. *Goddard* † in the Year 1669, where putting a Man's Arm into a glaſs Cylinder full of Water, the Water always ſunk when the Muſcles of the Arm were contracted, and riſe again to the firſt Standard when they were relaxed. This we think may be look'd upon as an *Experimentum Crucis*; whereas, if every Fibre was a Chain of Bladders, whoſe Contraction in Length aroſe from their Inflation in Breadth, all the World knows there would be a ſenſible Swell of the whole Arm upon muſcular Action.

LXII.

There are ſtill other Difficulties attending the veſicular *Hypotheſis*. If the animal Spirits are ſuppoſed to inflate the Cavities of the muſcular Fibres merely by a propulſive Force, like unto the Steam of boiling Water working in the Engine to raiſe Water by Fire, it ought to be proved from whence ſo ſtrong an Impulſe ſhould ariſe; and alſo how the Nerves, which are the Conduits thro' which this flatulent

† *Vide* Regiſter of the *Royal Society*, Vol. IV. p. 95.

flatulent Matter must be convey'd, should lie so loose and unelastic; it being evident from all Experience, that if such an elastic flatulent Vapour was to fly thro' the whole Length of the Nerves, with an Energy sufficient to give a Man a Power of lifting up great Weights, the Nerves must be stretched out in Proportion, and consequently would be very tense and elastic.

LXIII.

Those who suppose the Inflation of the Muscles to arise from a fermentative Motion in the Fluids, ought to prove, by a proper Number of Experiments, that there are Juices existing in the Body capable of such sudden and violent Rarefactions or Explosions, upon mixing with each other; and if this possibly could be done, the Diminution of the Bulk of the Muscles in Action, would overturn all their Scheme.

LXIV.

Hence it is evident that the *vesicular Hypothesis* ought to be intirely rejected, as being repugnant to the *Laws of Matter*, and to the *Phænomena* of the Muscles.

LXV.

By undoubted Experiments we are convinced, that the Intumescence of a Muscle, when it acts, arises merely from a Change made in its Figure; that is, as it shortens in Length the Belly grows thicker, and yet the Bulk in general is diminished.

Let us therefore inquire after the Agents which are capable of producing such surprising *Phænomena*, and at the same time shall be consistent with every other Operation in the Animal Oeconomy.

LXVI.

From what has been said it appears, that Contraction, or muscular Action, does not depend upon any Fluid dilating or distending the Fibres; but, on the contrary, they shrink up and grow less. The instantaneous Alternations from Constriction to Dilatation, and *vice versa*, manifestly discover that muscular Motion cannot be caused by such Juices as the *Blood*, *Lymph*, and such-like; but it must be from some more subtile *æthereal* Matter, which may be mix'd with the Blood in general, and secreted from it by the Glands of the Brain.

LXVII.

Let us but carefully consider the exquisite *Apparatus* of the Brain, the Quantity of Blood it receives, the infinite Number of its excretory Ducts, and the great Divisibility and Subtilty of Matter, and we shall find great Reason to conclude that there is a most subtil, æthereal, volatile Fluid, of great Force and Elasticity, perpetually secreted from the Blood, by the Glands of the Brain, and continually flying into the Nerves, for the Uses of muscular Motion, and many other great Purposes of the Animal Oeconomy.

LXVIII.

The delicate Texture of the Nerves, as well as that of the Brain, implies that the Fluid they convey to the Muscles must be exquisitely fine. Indeed when a Nerve is wounded, there flows from it a sweet, soft, clammy, balsamic Juice, which no doubt is carried, at all other times, by the evanescent Nerves to their ultimate Divisions, in order to nourish and preserve the most minute *Fibrillæ*, and all their Expansions; and this may properly be called the *Succus nutritius* of the Nerves. But I cannot conceive that this visible Juice has any thing to do with the immediate Cause of voluntary Motion; for so viscous a Matter could never admit of such sudden Vicissitudes, as are in muscular Action, if it was capable of performing it in other Respects.

LXIX.

There are Abundance of Considerations which evince the Existence of some subtil Spirit in the Nerves, much finer than to be the Object of our Senses. We have no Proof, either from Experiment or Reason, of any other instrumental or physical Cause of Sense or Motion, but this *animal Æther* which is elaborated from the Blood.

LXX.

The learned Dr. *Mead* * thinks no Regard ought to be had to the immechanical Notions of thoſe Authors, who imagine that there is no ſuch thing as a nervous Fluid in an animal Body; and that muſcular Motion and Senſation are performed only by the Vibrations of the Fibres of the Nerves, without the Intervention of any ſpirituous Fluid.

LXXI.

The ſurpriſing Diſcoveries which have been made of late Years, by a Variety of Experiments upon Electricity, do in ſome meaſure give us an Idea of the great Subtilty and Velocity of the nervous Fluid. I have been informed by the ingenious Mr. *Watſon*, a worthy Member of this *Society*, that the Swiftneſs of the electrical *Effluvia* is prodigious; that one Stroke of his Hand down the Tube, when well electrified, was felt as ſoon as his Hand could be at the Bottom of the Tube, through five Men ſtanding upon electrical Cakes, and communicating with each other by a Cane, Sword, or any other Non-electric.

LXXII.

Hence it follows, that if a Tube could be always excited, and was always to be applied to the End of a

* Introduction to his Eſſays on Poiſons, *Edit.* 3.

a proper Cord or String; the electric Matter, which is excited by Friction between the Hand and Tube, would ever be ready to exert its attractive Influence on Leaf-Gold, and such like Things, when placed within a due Distance of the End of the String; and perhaps this may be very similar to the Motion and Action of the nervous *Æther*.

LXIII.

Thus much being premised, and it being taken for granted, that we have an *ætherial Medium* in the Brain, Spinal Marrow, and all the *Capillamenta* of the Nerves, ever ready to be convey'd into the muscular Fibres, by the Power of the Will, and which *Medium* consisting of the most refined Matter in Nature; it follows, that the Motion of this nervous *Æther* may be as quick as Lightning, and also its attractive Power must be exceeding strong, by virtue of its vast Degree of Subtilty; as is evident from what Sir *Isaac Newton* * has calculated concerning the Rays of Light.

LXXIV.

From these Observations therefore, and from what has been said above concerning the Cohesion and Elasticity of the animal Fibres, I think we have great Reason to conclude, that muscular Motion does proceed from the Influence which the nervous *Æther* has

* Opt. *Qu.* 23, 24.

upon the component Particles of the muſcular Fibres themſelves, by inſtantly increaſing their attractive Virtue towards each, ſo as to make them run cloſer together, or, as it were, up into Heaps, as long as ſuch an additional attractive *Medium* is in the Fibres.

LXXV.

If we look back and conſider the Nature of the *Vis Reſtitutionis*, or Elaſticity in the muſcular Fibres, we ſhall find, that the Cauſe of that Property only wants to be increaſed, in order to overcome the diſtractile Force of the circulating Fluids, and the Reſiſtance of the antagoniſt Muſcles: Whence it follows, that if ſuch a Power was to be increaſed in one Set of Muſcles, and not in their Antagoniſts, thoſe Muſcles, whoſe elaſtic or retracting Power was increaſed, would be abbreviated in their Length, whilſt the others would be extended and lengthen'd.

LXXVI.

When any Muſcle is freed from the Power of its Antagoniſt, by a Wound, *&c.* it immediately contracts, and is not to be extended again by the Power of the Will. Whence it has been ſaid, that Contraction is the proper State of the Muſcles, and to which they always tend: But if we narrowly inſpect into this Affair, we ſhall find, that, when a Muſcle is contracted in this manner, it is no further ſo than the elaſtic reſtitutive Property in the Fibres is concerned. We do not find that ſuch a Muſcle is indurated, or its Belly ſwoln like unto what it is in

voluntary Action; for here being no Influx of the nervous *Æther* to increase the corpuscular Attraction, the Muscle is shortened only by the inherent mutual Attraction between the constituent Particles of its Fibres, without any Matter being superadded. This kind of Contraction therefore is evidently the State to which the elastic Fibres, tend by a continual *Conatus* in the component Particles to accede towards each other without the Assistance of the nervous *Æther*; so that this natural *Vis motrix* in the musculous Fibres is no more than what we mean by their Elasticity, or restitutive Property: It seems however to be demonstrated from hence, that muscular Action, and Elasticity in the Fibres, proceed from the same Cause in different Degrees; *viz.* from corpuscular Attraction.

LXXVII.

Let us now endeavour to corroborate these Arguments by some suitable Observations.

LXXVIII.

1*st*. From what has been said we may conceive more readily, than we know how to express, that the Will has a Power to direct the *æthereal Medium* contain'd in the Nerves, to any of the voluntary Muscles, with such a Degree of Celerity as it pleases; and to stop the Influx as suddenly.

LXXIX.

2*dly*. It is evident that the Coats, or carnous Substance of every Fibre, must necessarily increase in its Thickness,

Thickneſs, when it abates in its Length; and what Power can produce this Effect, but ſuch a one as increaſes the mutual Attraction between the conſtituent Particles?

LXXX.

3*dly*. Hence appears the Reaſon, why the Belly of a Muſcle ſwells during its Contraction, notwithſtanding its Dimenſion in general is diminiſh'd; for as the component Particles of each Fibre are more looſely join'd together about the Middle than towards its Extremities, which are generally tendinous, it is natural to ſuppoſe that the chief Action is between them; that is, when a Fibre grows ſhorter, ſuch of its Particles which are moſt at Liberty run nearer together, and as the Motion of all Bodies is ever in proportion to the Impulſe they receive, and the Reſiſtance they meet with, ſo when the conſtituent Particles of the muſcular Fibres are drawn into a ſhorter Compaſs, by the means above-mention'd, the Middle of the Fibres muſt ſwell either inwardly or outwardly, or both, according to the Reſiſtances they meet with.

LXXXI.

And *laſtly*, Since the Coats of the muſcular Fibres do moſt certainly grow thicker as they contract in their Length, and yet the external Surface of the Muſcle in general is diminiſhed; it manifeſtly follows that their Cavities muſt grow leſs, and their

contain'd Fluids must be pressed out, in proportion to the Contraction of the Muscle.

LXXXII.

This appears upon Blood-letting, when squeezing any thing hard in the Hand will make the Blood fly out with a greater Velocity, and thereby form a larger *Parabola*.

LXXXIII.

This also accounts clearly for the Induration and Paleness of a Muscle during its Action.

LXXXIV.

And again, it follows hence, that in the Action of the Muscles there is an alternate *Diastole* and *Systole* perfectly analogous to the Action of the Heart, which greatly contributes towards pushing on the Blood in the Veins.

LXXXV.

The Muscles being contracted merely by the Influence of the nervous *Æther*, and the Influx of the *Æther* being stopt by withdrawing the *Impetus* given to it by the Power of the Will; the Reason and Manner of their Relaxation will easily appear. For since the nervous Fluid is extremely subtile, that Portion of it which is thrown into the muscular Fibres, acts but for a Moment, or the least

Space

Space of Time, ſo quick is it in its Motions, and ſo penetrating in its Nature; and no ſooner is the Vigour of the Attraction over, but the Tenſion of the Antagoniſt Muſcles, and the Impulſe of the Blood will extend them again.

LXXXVI.

Whoever duly conſiders the well known Effects of magnetical and electrical *Effluvia* will be at no Loſs to conceive the inſtantaneous Influence which the nervous *Æther* has upon the muſcular Fibres.

LXXXVII.

It muſt be confeſs'd indeed, that theſe *Intima Naturæ*, or ſecret Operations in the Animal Oeconomy are all skreen'd from our Knowledge, the Agents being too ſubtil ever to become the Objects of our Senſes, though ever ſo well aſſiſted; ſo that we can only form our Schemes, and deduce our Arguments from ſuch collateral Proofs, or from ſuch *Data* as we are pretty ſure are true. As for Inſtance; the Influence which the *Soul* has upon the *æthereal Medium* in the Nerves muſt be by Impulſe; for though our finite Capacities are not able to comprehend the Nature of immaterial Impulſe; yet nothing is more certain than that the moſt ſubtil Matter in the Univerſe cannot be moved without ſome impreſſed Force.

LXXXVIII.

LXXXVIII.

That the Will does exert itſelf after this manner, is in a good meaſure proved by Dr. *Stuart*'s * Experiment upon a Frog, where a proper Impulſe being given to the *Medulla ſpinalis* did excite Motion in the voluntary Muſcles, though the Head was ſever'd from the Body.

LXXXIX.

Hence alſo it appears, that the Nerves are always replete with a ſubtil Fluid capable of contracting the Muſcles, or otherwiſe ſuch an Impulſe on the Beginning of the Nerves, could not have excited Motion after the Head was cut off.

XC.

And again, common Experience aſſures us, that tho' the Nerves are always replete with an *ætherea l Medium*, yet this *Medium*, in a State of Health, never flies out at their Extremities, into the muſcular Fibres, without ſome Impulſe by the Direction of the Will: Whenever it happens to do ſo, Convulſions and Cramps are the natural Conſequences.

XCI.

* Lectures on Muſcular Motion.

XCI.

It may perhaps ſeem ſtrange to ſome, that I have not all this while taken any Notice of the Blood, as an Agent in muſcular Motion; ſince it has ever been reckoned ſome way neceſſary towards it. But notwithſtanding this Opinion has been ſo long and ſo generally received, yet if our Scheme be the true one, it evidently appears the Blood hath nothing to do with the immediate Contraction of the Muſcles.

XCII.

From the cloſe Connection of the nervous *Capillamenta* in all or moſt of their Ramifications, to thoſe of the Arteries, it ſeems as if the *Diaſtole* and *Syſtole* of the arterial Syſtem was ſome how uſeful to them. Perhaps it may aſſiſt in puſhing on the *Succus nutritius*, or that clammy balſamic Juice which is in the Nerves, towards their Extremities; but I cannot conceive that the Blood itſelf is in any way aſſiſting towards muſcular Motion, except it be by keeping the Fibres warm, ſupple, diſtended, and every way ready for the Influx of the nervous *Æther*.

XCIII.

I have tied up and cut aſunder both the *Carotid* and both the *Crural* Arteries of the ſame Dog, without deſtroying the Motion of one Muſcle. Nothing leſs than laying a Ligature on the *Aorta deſcendens* will

will deſtroy the Motion of the hinder Parts; and poſſibly this may happen from the great Diſtenſion of the *Aorta* above the Ligature, preſſing upon the Nerves which go to the lower Parts.

XCIV.

It is certain indeed, when all the Blood is intercepted the Fibres will ſoon collapſe, and grow flaccid, and muſcular Motion will ceaſe, merely for Want of the Warmth, Suppleneſs, and Diſtenſion which the Muſcles receive from the Blood. But what I think moſt reaſonable is, that the Blood is no Way concerned as an efficient Cauſe in purſing up and contracting the Fibres; it rather by its Motion through the Muſcles, acts as an Antagoniſt to their Contraction, by extending and diſtending them; for the Blood, by the *Diaſtole* and *Syſtole* of the Arteries, is continually urging on its Paſſage through the Muſcles.

XCV.

Thus I have endeavour'd to deduce and illuſtrate the Cauſe of muſcular Motion from true Principles, by purſuing only thoſe *Laws of Nature*, which our great Philoſopher Sir *Iſaac Newton* has in ſo ſurpriſing a manner diſcover'd to us. But I am far from thinking this a complete Account; I know it requires more Experiments, and better Reaſonings than I am Maſter of, to explain it as it ought; and even after all, there are, and ever will be, ſome Things above the Reach of our Capacities to demonſtrate,

ſtrate, any otherwiſe than by their Effects, or ſecond Cauſes. Such are, the Nature of an immaterial Impulſe; the real Exiſtence of ſo ſubtil a Fluid as is attributed to the Nerves; and the true Cauſes of Attraction and Repulſion.

XCVI.

That the SUPREME BEING hath implanted an immaterial Spirit in every living Creature, for the Purpoſes of Senſation and voluntary Motion, I think cannot be denied by any one in his Senſes: But perhaps it may not become us to be too ſollicitous about the *Modus* of Action betwixt the *Soul* and *Matter*; theſe Things being above the Reach of human Reaſon. It is ſufficient for our Purpoſe, that we know the Will has a Power of determining the nervous *Æther* immediately and directly to every individual voluntary Muſcle.

XCVII.

The Exiſtence or Non-Exiſtence of the nervous Fluid, commonly called the animal Spirits, has been a Controverſy of long ſtanding. The firſt Searchers into the Structure of the human Body ſoon found that muſcular Motion depended upon the Nerves, or ſomething within them; and this has conſtantly been aſſerted, and admitted as a known Truth. The Advocates for the Exiſtence of animal Spirits have generally ſuppoſed that voluntary Motion was performed by a ſudden Inflation of the Muſcles, either by the Power of the nervous Fluid itſelf, or by an inſtantaneous Ferment with ſome other Fluid; and I am apt to believe, that this Doctrine proving

inconſiſtent with many Things relating to the Animal Oeconomy, and contrary to ſome known Experiments, might give Riſe to the vibrating Scheme, where the Exiſtence of the animal Spirits is denied, and where it is ſuppoſed that both Senſation and muſcular Motion may be performed merely by the Elaſticity of the Nerves, and Contractions firſt of all begun in the Brain, and ſo communicated to the fleſhy Fibres: But this is ſo immechanical a Notion as not to deſerve an Anſwer; it being impoſſible for a vibrating Motion in one Cord or String, were it ever ſo elaſtic, to cauſe a Contraction in another, without the Intervention of ſome Fluid.

XCVIII.

I ſhall conclude this Lecture therefore with obſerving, that the Exiſtence of an *æthereal Medium* in the Nerves is paſt all manner of Doubt; it being otherwiſe contrary to the known Laws of Nature for the Nerves to be the Cauſe of muſcular Motion if they were ſolid, or did not admit the moſt ſubtil Fluid, ſecreted by the Glands of the Brain, to paſs through them.

And ſince it is known from Experiment, that the Muſcles grow leſs in Action, and, conſequently, the conſtituent Particles of every Fibre muſt run nearer together before ſuch a *Phænomenon* can happen; we think it very manifeſt that this Property of Conſtriction ariſes from the Principle of corpuſcu-

lar

lar Attraction being increaſed and ſtrengthened by the Influence of the nervous *Æther*; a Principle, which, from the endleſs Diviſibility and Subtilty of Matter, we may never be able to comprehend, though we know it to exiſt in Nature from innumerable Obſervations and Experiments.

The End of the Second Lecture.

LECTURE III.

XCV.

Read April 9. 1747.

IN Confirmation of the Truth of what has been ſaid in the two preceding Lectures, and for a further Illuſtration of this Subject of muſcular Motion, I beg Leave to offer ſome Thoughts concerning the Cauſe and Manner of Action in the involuntary Muſcles; and after that I ſhall relate ſome Experiments, which I have made in order to illuſtrate our Theory in general, as far as the Nature of the Subject will admit.

XCVI.

When any Muſcle, voluntary or involuntary, is fully contracted, that is, when its component Particles are drawn into the cloſeſt Contacts they are capable of, by the Influence of the *æthereal Medium* in the Nerves, it is evident, from all the Laws of Matter, that they would not recede from each other again without ſome impreſſed Force. Now in all the voluntary Muſcles we very well know, that when one Set of them are contracted, their Antagoniſts are lengthen'd, and *vice verſa*; ſo that the *Vis Reſtitutionis* in all the ſtretched-out Fibres, and the *Momentum*

Momentum of the Fluids to enter again into the contracted Muſcles, will inſtantly dilate and diſtract them, when the Impulſe of the Will ceaſes, till an exact Equilibration is reſtored.

XCVII.

This being the plain Matter of Fact in regard to the voluntary Muſcles; let us now endeavour to find out the Mechaniſm belonging to the involuntary ones, and more particularly of that very curious one the Heart.

XCVIII.

Dr. *Houdly*, in his *Lectures on Reſpiration*, has manifeſtly ſhewn that the external Ranges of the intercoſtal Muſcles appear to have all the characteriſtic Marks of antagoniſt Muſcles. If you take three Ribs together, *ſays he*, and obſerve the different Ranges of the Fibres in the two intercoſtal Muſcles, which lie on each Side the middlemoſt of theſe Ribs; you will ſee, at firſt Sight, that the internal Range in the lower Muſcle, counter acts directly the external Range in the upper Muſcle.

XCIX.

The Action of the *Diaphragm* is alſo counterbalanc'd by the abdominal Muſcles, and Contents of the *Abdomen*; which ſqueeze it up, in a convex Form, into the Cavity of the *Thorax*, upon every Expiration.

C.

As to the *Sphincter* Muſcles, they ſeem to be always in the ſame natural contractile State; and whenever they are ſtretched out, by ſome ſuperior Power, they recover their uſual Dimenſions merely by their reſtitutive or elaſtic Property. The ſame may be ſaid of the *Stomach*, *Uterus*, *Veſica*, which contract by the ſame means, into a narrower Capacity, as ſoon as their Contents are diſcharged.

CI.

I ſhall confine my Thoughts therefore to the Action of the Heart only, as being the moſt perfect involuntary Muſcle.

CII.

The Heart is a *Viſcus* which has given the *Literati* a great deal of Trouble to find out its real Mechaniſm, and the true Cauſe of its regular Alternations of Contraction and Dilatation. I have oftentimes laid open the Breaſt of a Dog, and kept his Lungs playing with a Pair of Bellows, in order to obſerve how regularly and alternately the *Syſtole* and *Diaſtole*, both of the Heart and its Auricles, followed each other; that is, when the Ventricles were contracted, the Auricles were dilated, and *vice verſa*; ſo that the Auricles ſeem, in ſome meaſure, to act as Antagoniſts to the Ventricles, and the refluent Blood may be of the ſame Service to the Auricles.

icles. But there being so much Disparity between the contractile Strength of the Ventricles and that of the Auricles, there must necessarily be some other Cause, which, when the Heart is fully contracted, makes it unbend again, or cease to contract; by which means the Auricles, though so very weak in comparison of the Ventricles, have Power enough to throw in Blood, and thereby to distend the Ventricles to a certain Degree, before they are capable of acting again.

CIII.

If we might be allowed to make an Estimate of the Difference between the contractile Strength of the Heart and its Auricles, from the Difference of their Bulk, or Quantity of Fibres; we should find it to be about *nine* to *one*; as I have observed, by weighing the Hearts of several Animals, and their Auricles separately.

CIV.

Hence it does not seem reasonable to think, that the weak Efforts of the Auricles would be sufficient to cause the *Diastole* of the Heart, without some other Assistant.

CV.

One great Use of the Auricles is, to receive a Quantity of Blood, during the *Systole* of the Heart; sufficient to fill the Ventricles again at their *Diastole*.

stole. The same may be said, in some measure, of the Veins nearest the Heart, which may be plainly seen to dilate during the Contraction of the Auricles; so that the Heart, Auricles, and Veins, have all their *Systoles* and *Diastoles* in subordinate Degrees.

CVI.

Without such Receptacles as these it would be impossible for the Ventricles of the Heart to be fill'd from the Veins so suddenly as they are; for though the *Areas* of the transverse Sections of the Veins are much larger than those of the Arteries, yet we find, by Dr. *Hales's Hæmastatical Experiments*, that the Velocity of the Blood in the Arteries is above *six* to *one* to that in the Veins: But since the Auricles keep receiving the Blood whilst the Heart is in its *Systole* (the Veins doing the same by the Auricles) a due Quantity of Blood is always ready to be thrown into the Heart, by the Time it is fit to receive it; for the *Diastole* takes up two Thirds of the Space of Time between each Pulse.

CVII.

Here then we may observe that the *Momentum* of the refluent Blood acts upon, and distends the Veins nearest the Heart, whilst the Auricles are contracted; that it also dilates the Auricles, whilst the Heart is in Action; and that the Heart is in its *Diastole*, whilst the Auricles are in their *Systole*.

But

But the great Difficulty in accounting for the *Diastole* of the Heart, is from the Disproportion between its contractile Power, and that of the Auricles; it being plain, from what has been said above, that the *Momentum* of the Blood in the Veins is stopt by the Contraction of the Auricles, so that the Ventricles of the Heart can receive no other Impulse from the Blood at that time, than what is derived from the Contraction of the Auricles.

CVIII.

Hence therefore it follows, that if the *Systole* was the natural State of the Heart, and to which it always tended with its full contractive Power, the Impulse of the Blood, from the Contraction of the Auricles, could never be able to dilate it.

CIX.

Dr. *Lower* makes the *Systole* the natural State or Action of the Heart, and the *Diastole* the violent one: *Boerhaave*, on the contrary, makes the *Systole* the violent, and the *Diastole* the natural State. But perhaps neither of these Opinions may be right, in the strictest Sense; for if we look back we shall find, that if the Fibres were not tense they could not be elastic; and if some Violence was not put upon them, by the Impulse of the circulating Fluids, they would not be tense: Hence it appears, that Elasticity proceeds from a State of Violence.

CX.

And again; When a Muſcle is freed from the Power of its Antagoniſt, and is thereby left at full Liberty to contract, as it always will by its elaſtic, reſtitutive Property only, it does ſo no further than in Obedience to the common Power of Attraction between its component Particles; but in all other muſcular Action, when this attractive Power is greatly increaſed by the Influx of the nervous *Æther*, the Conſtriction is carried much further, and the Muſcle is more fully contracted than it ever is in the other State: Hence it follows, that Contraction, in its fulleſt Degree, is not the natural State of a Muſcle.

CXI.

And further; When the conſtituent Particles of the Fibres are drawn into their cloſeſt Contacts, by the Influx of the nervous *Æther*, it requires ſome Force, in a contrary Direction, to elongate the Fibres again; ſo that Extenſion, or Dilatation is alſo a State of Violence.

CXII.

From the Whole then it appears, that neither the *Syſtole* nor *Diaſtole*, in a full Degree, is the natural State of the Heart; and this we ſhall ſhew more plainly hereafter by inſpecting the Hearts of Animals after being bled to Death. But to proceed:

CXIII.

From what has been ſaid above, it ſeems reaſonable to conclude, that if Contraction, in its fulleſt Degree, was the natural State of Reſt or Quieſcence in the Heart; the *Momentum* of the Blood from the Contraction of the Auricles, could not be a ſufficient Counterpoiſe. And ſince ſo many fruitleſs Attempts have been made to account for the *Diaſtole* of the Heart; from the *Impetus* of the Blood in the Veins, and from the Preſſure of the Atmoſphere, *&c.* give me Leave to propoſe the following *Queries*.

CXIV.

1*ſt*. May not the Heart be a compound Muſcle; that is, may it not have its Antagoniſt within itſelf? Or, in other Words, are not ſome of its Fibres ſo ranged, that whilſt one Set of them is contracted and ſhorten'd, others may be ſtretched out; analogous to the Action of the intercoſtal Muſcles, or any other Muſcles with their Antagoniſts?

CXV.

2*dly*. Is the nervous *Æther* tranſmitted from the Brain to the Heart in a pulſatory Manner, at equal Diſtances of Time; or may it be ſuppoſed to move uniformly through the Nerves, and ſome Interruption is given to its Influx into the muſcular Fibres, when the Heart is in its *Syſtole?*

CXVII.

3dly. Does not the *Diaſtole* of the Heart depend upon an Abatement of the Tenſion in the contracted Fibres; a Motion of Reſtitution in ſuch as are over ſtretched; and the Influx of the Blood conjunctly?

CXVIII.

Without ſome ſuch Mechaniſm as this, no Power that we know of, belonging to the animal Oeconomy, would be able to cauſe the *Diaſtole* of the Heart: But if ſuch a Structure, as is above-mention'd, could be proved, no more Difficulty would attend the Explication of it than that of the voluntary Muſcles.

CXIX.

In diſſecting the Heart we find a great many different Orders or Series of Fibres, variouſly contorted, and running in contrary Directions; ſo that, for ought we know, it may be an *Epitome* of muſcular Conſtruction in general.

CXX.

We may here obſerve what Care *Nature* has taken to prevent too great a Dilatation in the *Diaſtole* of the Ventricles, the right one eſpecially, as being the weakeſt, by forming the *Papillæ*, or *Columnæ*, which run from

from the *Septum*, or middle Partition, to its opposite Sides; whereby they act as ſo many Braces in the *Diaſtole*, and when they contract, they alſo aſſiſt in the *Syſtole*. And perhaps it may not be the moſt improbable Conjecture to think; that as much Care may have been taken in providing a ſufficient Number of Fibres, or little Muſcles, which may be ſo formed as to act in the *Diaſtole*, as Antagoniſts to thoſe which occaſion the *Syſtole*.

CXXI.

The Hearts of Frogs, Vipers, Eels, *&c.* ſeem to evince the Reaſonableneſs of this Conjecture, by continuing their *Syſtoles* and *Diaſtoles* after they are taken out of the Body; when there is no refluent Blood to dilate the Ventricles, and, conſequently, if there were no Fibres upon the Stretch, when the Heart is contracted, which, by their Elaſticity, or reſtitutive Power, did pull back, or elongate ſuch as were contracted, there could not poſſibly be any *Diaſtole* in ſuch Circumſtances.

CXXII.

We are aſſured by Mr. *Boyle*, in his *Phyſico-Mechanical Experiments*, that the Heart of an Eel hath continued to beat an Hour, in an exhauſted Receiver; after which, finding its Motion very languid, and almoſt ceaſed, by breathing a little upon that Part of the Glaſs where the Heart was, it quickly regain'd Motion; and an Hour after that,

finding

finding it almoſt quite gone, he was able to renew it, by the Application of a little more Warmth.

CXXIII.

The ſame *illuſtrious Author* further aſſures us, that he has ſometimes cut the Heart of a Flounder tranſverſely, into two Parts; and freeing each from the Blood it contain'd, he obſerved, for a conſiderable Time, that both of them together continued their former Contraction and Relaxation. And once, thus cutting one into ſeveral Pieces, he found, to his Surprize, that they not only moved as before, but that even the Whole, thus ſeparated, long preſerved the ſame Succeſſion of Motion, as appeared therein whilſt coherent.

CXXIV.

Now can the *Diaſtole* of the Heart, in theſe Experiments, be accounted for upon any other Principles than thoſe we have laid down? Here was no Impulſe from the refluent Blood to dilate the Auricles, or to diſtend the Ventricles. The Preſſure of the Atmoſphere was alſo quite taken away in the firſt Experiment, and could not poſſibly be of any Service in the laſt; and yet the *Diaſtole* continued.

CXXV.

Can the ſame Fibres which are contracted, have it in their own Power to fly out again to their uſual Lengths? If this cannot be, what Power is there

there in a Heart, taken out of the Body, to unbend itſelf, or to dilate its Ventricles, after they are once contracted, unleſs we ſuppoſe ſome of its Fibres to act as Antagoniſts to others?

CXXVI.

We have good Reaſon therefore to believe that ſome Fibres of the Heart are always ſtretched out beyond their natural Tone, when others are contracted; ſo that, by their elaſtic, reſtitutive Property, they do act as Antagoniſts in a certain Degree.

CXXVII.

In regard to the *Syſtole*, in ſuch Hearts as are taken out of the Body, and cut into ſeveral Pieces, we conceive, that as long as Warmth and Moiſture remain, ſo long may the *æthereal Matter* in the Nerves, continue to fly into the Fibres, and contract them; and when it ceaſes, more Warmth, or a gentle Impulſe (even with the Point of a Needle only) will revive the Motion.

CXXVIII.

Hence we may, in ſome meaſure, diſcover the amazing Subtilty of the nervous *Æther*; when ſuch very ſmall Sections of the Nerves, as in the above-mention'd Experiment upon the Heart of a Flounder, ſhould contain Matter ſufficient for ſo many Contractions.

CXXIX.

CXXIX.

Let us now return to our *ſecond Query*, and examine whether the nervous *Æther* is tranſmitted from the Brain to the Heart, in a pulſatory Manner, at equal Diſtances of Time; or whether ſome Interruption is only given to its Influx into the muſcular Fibres, when the Heart is in its *Syſtole*.

CXXX.

In order to underſtand this, we muſt look back and conſider, that, according to our Theory, if the *æthereal Medium* in the Nerves was perpetually flying into the muſcular Fibres of the Heart, it would be conſtantly contracted, notwithſtanding the *Momentum* of the Blood, the Contraction of the Auricles, or the *Vis Reſtitutionis* in the ſtretched-out Fibres. Hence then it is evident, that the alternate Contractions and Dilatations of the Heart proceed from an alternate Influence of the nervous *Æther*; but how this Alternation happens, when the Nerves which ſupply the Heart are not, in the leaſt, under the Direction of the Will, is the Difficulty we labour under.

CXXXI.

In all the Nerves which ſupply the voluntary Muſcles, it is certain there are Reſtrictions in ſome Parts of them which the *æthereal* Matter is not able to dilate without an additional Impulſe from the Will;

Will; or otherwise Cramps and Convulsions would perpetually happen. But in those which supply the Heart, the Motion of the *Æther* through them cannot have any Assistance or Impulse from the Will, as not being in the least under its Influence; so that the Vibrations of the *Meninges* of the Brain, and the Dilatations of the Arteries may be supposed to be the Agents which propel the *Æther* towards the Heart.

CXXXII.

Hence then it seems to follow, that the alternate Contractions of the Heart may proceed from the alternate Impressions made on the Nerves, by the *Meninges* of the Brain, and Dilatations of the Arteries.

CXXXIII

Upon opening the Sculls of living Animals, the *Dura Mater* may plainly be seen to have its *Systoles* and *Diastoles* corresponding to those of the Arteries; but since the Heart continues to beat after the Head is cut off, or even after it is taken out of the Body, where there cannot be any alternate Succussions made on the Nerves by the *Meninges* of the Brain, or by the Pulsation of the Arteries, it seems reasonable to believe that this Alternation is occasion'd by some Impediment being given to any further Influx of the *Æther* into the muscular Fibres of the Heart, when it is fully contracted; or otherwise, the Heart would constantly remain in a

State of Contraction, as long as there was any *ethereal Matter* flying from the Nerves.

CXXXIV.

If we consider in how many different Directions the muscular Fibres of the Heart run, how much they are corrugated, thickened, and swelled, when fully contracted, and how strong and uniform the Pressure must be in their greatest Degree of Action; it may not perhaps appear unreasonable to think that the Extremities of the Nerves, which are inserted into every Fibre, and which are extremely small and tender, may be pressed upon and squeezed, so as to prevent the Influx of the *Æther*, till the Pressure is abated, or till the Fibres are extended again to their usual Lengths.

CXXXV.

What seems to evince the Reasonableness of this Supposition is the Nature of the *Shaking Palsey*; where the voluntary Muscles immediately become involuntary ones, so far at least, as to be alternately contracted and relaxed without the Consent or Direction of the Mind.

CXXXVI.

Now this Distemper we believe to proceed from a particular Weakness in the Nerves, whereby those little Restrictions in them, which keep the *ethereal Matter* within due Bounds, in a State of Health, are

are so far weaken'd and destroyed, that the *Æther* has a Power of flying into the muscular Fibres without any Impulse or Direction from the Will, after the same manner it does into the Heart. If therefore no Interruption was to be given to the Influx of the nervous *Æther* by the Muscles themselves, when they were contracted, it would follow, that if the *Flexors* of any Limb were to be first contracted, the *Extensors* would not be able to recover the *Æquilibrium*, and to be contracted in their Turn, without the Assistance of the Will; for if our Doctrine be right, *viz.* that muscular Motion proceeds from the constituent Particles of the Fibres being drawn into closer Contacts by the attractive Influence of the nervous *Æther*, it necessarily follows, that if the same Quantity of *Æther* was continually to fly into a Muscle already contracted, it would have a greater Influence on the component Particles so approximated, than on others in the antagonist Muscles, which are distracted, and consequently touch each other in fewer Points.

CXXXVII.

Hence it seems to be evident that some Impediment is given to the Influx of the nervous *Æther* when the Muscles are contracted, or otherwise that they would always remain so; for, though the Will may be able to remove such little Impediments, and to keep the voluntary Muscles in a State of Contraction for a considerable Time; yet since the voluntary Muscles, when affected with a Palsy, are regularly and alternately contracted, and have their

Systoles and *Diastoles* analogous to the Heart and its Auricles, I think we have good Reason to conclude, that the same Principles which contract and dilate the voluntary Muscles, in the above-mention'd Condition, are the Principles which occasion the *Systole* and *Diastole* of the Heart, with this Difference only, that the Influx of the Blood into the *Sinuses* of the Heart, is what no other Muscle in the Body has or receives; and without Doubt this may truly be reckon'd the greatest Assistant in dilating the Ventricles of the Heart, after the attractive Influence of the nervous *Æther* ceases, and the *Æquilibrium* is restored between the stretched-out Fibres and those which were contracted.

CXXXVIII.

What has been said seems greatly to evince the Truth of our *third* and *last* Supposition; *viz.* that the *Diastole* of the Heart may depend on an Abatement of Tension in the contracted Fibres, a *Vis Restitutionis* in such as are over-stretched, and the Influx of the Blood conjunctly.

CXXXIX.

Give me Leave to relate two or three Experiments which I have made, in order to illustrate the foregoing Theory, and then I shall conclude.

CXLI.

EXPERIMENT I.

Having obſerved that the Ventricles of the Hearts of all Animals, when bled to Death, are dilated, or ſtand wide open to a certain Size, though there happens to be very little Blood in them, it ſeems as if the *Diaſtole*, in a certain Degree, was the laſt Motion of the Heart. In order to know the Truth of this, I open'd the *Thorax* of a Dog, and kept his Lungs playing with a Pair of Bellows, that I might perfectly ſee the ſeveral Actions of the Heart and its Auricles.

CXLII.

The Auricles ſeem'd to me to begin the Motion, and the *Syſtole* of the Heart always inſtantly followed that of the Auricles. Then the *Apex* and Sides of the Heart ſunk down, and were lengthen'd of their own accord, before any Blood was thrown into the Ventricles, from the Contraction of the Auricles; for there was oftentimes more than double the Time taken up in the *Diaſtole* of the Heart, than the *Syſ-tole* both of the Auricles and Ventricles required.

CXLIII.

If I deſiſted from blowing freſh Air into the Lungs for ſome little Time, the Heart would lie ſtill, and yet I could recover its Motion again, by ſtrongly

ſtrongly diſtending the Lungs. In this Action I never could diſcern that the Heart began the Motion, but the Auricles always contracted firſt, and then the Heart immediately afterwards; though, at laſt, I ſaw ſeveral Contractions of the Auricles, which were not ſucceeded by any Motion of the Heart.

CXLIV.

I muſt not forget to mention, that in another Dog I ſaw ſeveral *Syſtoles* and *Diaſtoles* of the Heart after I had purpoſely cut aſunder both the *Vena Cava's*; and could plainly perceive the Ventricles to relax and open themſelves, when very little or no Blood could poſſibly flow into them. This however is much more eaſily ſeen in the Heart of a Viper, when taken out of the Body, which will continue its *Syſtole* and *Diaſtole* for a long time; and if it ceaſes to beat ever ſo often, and you renew its Motion again and again, by breathing upon it, *&c.* You may obſerve that the Auricle always contracts before the Ventricle, and that the *Diaſtole* is the laſt Motion, though there is no Blood to diſtend the Heart.

CXLV.

The Capacity of the right Ventricle of the Dog's Heart, in the above Experiment, as it open'd of its own accord, was ſomething more than a cubic Inch, as I found, by pouring in melted Wax from a Ladle, without any Preſſure from a perpendicular Height,

Height, after the Auricle and Blood-veſſels were cut off tranſverſely.

CXLVI.

Experiment II.

Taking the Heart out of an Ox as ſoon as I poſſibly could after he was kill'd, and having cleanſed it from the Blood, by waſhing it in warm Water, I fill'd both the Ventricles with melted Wax, without any diſtending Force more than by pouring it from the Ladle. The Heat of the Wax ſeem'd rather to contract the Fibres, and to leſſen the Capacities of the Ventricles; for ſome of the Wax kept running out from the Heart after I had done pouring it from the Ladle.

CXLVII.

The Capacity of the right Ventricle was equal to $5\frac{1}{2}$ cubic Inches. The Capacity of the left Ventricles was not quite 5 cubic Inches.

CXLVIII.

From theſe Experiments it manifeſtly appears, that the laſt or reſtitutive Motion of the Heart is to dilate or open the Ventricles, and that without any Aſſiſtance or Impulſe from the refluent Blood.

CXLIX.

Dr. *Hales* * injected the left Ventricle of the Heart of an Ox from a Column of melted Wax $4\frac{1}{2}$ Feet high, which distended the Capacity of it to 12. 5. cubic Inches. Now supposing the Impulse of the refluent Blood to be equal to a Column of melted Wax $4\frac{1}{2}$ Feet high, and that in every *Diastole*, whilst the Ox was alive, the left Ventricle was distended to the Capacity of 12. 5. cubic Inches, yet we may observe that the Heart, immediately after each *Systole*, relaxes and dilates its Ventricles to a certain Degree, by its own Mechanism, and to whatever Capacity the Ventricles are afterwards dilated, it must necessarily proceed from the Impulse of the Blood only; all which is perfectly agreeable to our *third Query*, *viz.* that the *Diastole* of the Heart may depend upon an Abatement of the Tension in the contracted Fibres, a Motion of Restitution in such as are over-stretched, and the Influx of the Blood conjunctly.

CL.

EXPERIMENT III.

I took the Heart out of a live Viper, and placing it upon a Piece of writing Paper, I found it beat at

* Hæmastatics, *pag.* 25.

at the rate of 24 or 25 Pulſations in a Minute, for three or four Minutes. After this I laid the Paper upon the Palm of my Hand, the Warmth of which increaſed the Number of Pulſes to 37 the firſt Minute, and to 48 the ſecond. Laſt of all I put it into warm Water, a Degree or two warmer than the Blood, where it beat 87 Pulſes the firſt Minute, and afterwards declined in its Motion more and more till it quite ceaſed.

CLI.

May we not from hence obſerve what vaſt Influence *Heat* had upon the nervous *Æther* to make it fly ſo quick into the Heart as to increaſe the Pulſe to almoſt four times their uſual Number? How far the Heat in ſome ſort of Fevers may quicken the Pulſe, is not to our preſent Purpoſe to enquire.

CLII.

It is amazing to think that ſuch very ſmall Portions of the Nerves, as were cut out with the Heart in this Experiment, ſhould contain Matter ſufficient for ſo many Contractions; and that it did not all of it inſtantly fly out from the wounded Ends, even before it was put into warm Water.

CLIII.

How far theſe Experiments may ſerve to illuſtrate and confirm the Truth of our Theory of muſcular Motion, is very humbly ſubmitted to the candid

Judgment of this *Honourable* and most *Learned Society*; and I shall think myself well recompensed for my Pains, if it so far meets with their Approbation as not to contain any thing inconsistent with true Philosophy, or to any of the known Laws of the Animal Oeconomy.

FINIS.